Psychological Bulletin

CONTENTS

ARTICLES:

Effects of Early Experience upon the Behavior of Animals ..... FRANK A. BEACH AND JULIAN JAYNES 239

Sequential Analysis in Psychological Research ..... DONALD W. FISKE AND LYLE V. JONES 264

Speed of Learning and Amount Retained: A Consideration of Methodology ..... BENTON J. UNDERWOOD 276

SPECIAL REVIEW:

Some Recent Books on Personality ..... DAN L. ADLER 283

BOOK REVIEWS:

Buros' The Fourth Mental Measurements Yearbook ..... IRVING LORGE 297

Mowrer's Psychotherapy, Theory and Research ..... E. LOWELL KELLY 299

Lacey's Statistical Methods in Experimentation; Lindquist's Design and Analysis of Experiments in Psychology and Education; Walker and Lev's Statistical Inference ..... LEONARD S. KOGAN 303

Lehman's Age and Achievement ..... WAYNE DENNIS 306

Gray's Psychology in Industry ..... D. J. MOFFIE 308

Daniel and Louttit's Professional Problems in Psychology ..... DAEL WOLFLE 309

Cabot and Kahl's Human Relations ..... HAROLD PROSHANSKY 310

Whiting and Child's Child Training and Personality: A Cross-Cultural Study ..... NANCY BAYLEY 313

Rosen's Direct Analysis: Selected Papers ..... M. ERIK WRIGHT 315

Viteles' Motivation and Morale in Industry ..... WILLIAM MCGEHEE 315

Walter's The Living Brain ..... DONALD B. LINDSLEY 317

Festinger and Katz's Research Methods in the Behavioral Sciences ..... ROGER W. RUSSELL 318

Sarason's Psychological Problems in Mental Deficiency ..... KARL F. HEISER 320

Committee on Colorimetry of the Optical Society of America's The Science of Color ..... DEAN FARNSWORTH 321

Moreno's Who Shall Survive? ..... LEON FESTINGER 322

Morse's Satisfaction in the White-Collar Job ..... WILLIAM MCGEHEE 323

BOOKS AND MONOGRAPHS RECEIVED 325

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Psychological Bulletin

EFFECTS OF EARLY EXPERIENCE UPON THE BEHAVIOR OF ANIMALS

FRANK A. BEACH AND JULIAN JAYNES
Yale University

It has always been apparent that human beings learn a great deal during infancy and childhood and that the results of this learning often continue to affect behavior throughout the remainder of the life span. Long before the days of a science of behavior, naturalists speculated about the possible importance of early learning in the lives of lower animals. More than two thousand years ago Aristotle wrote:

> Of little birds, some sing a different note from the parent birds, if they have been removed from the nest and have heard other birds singing; and a mother nightingale has been observed to give lessons in singing to a young bird, from which spectacle we might obviously infer that the song of the bird was not congenital with mere voice, but was something capable of modification and improvement (**5**, p. 121).

During the two thousand years after Aristotle other writers compiled anecdotes and life histories of various animal species, but not until the emergence of comparative psychology in the closing years of the nineteenth century did the subject of behavioral development attract serious scientific interest. Among the earliest American workers to deal with this problem was Small, who in 1899 published an article on the albino rat as a useful subject of psychological investigations (**119**). Small's account included a section dealing with behavior in infancy and adolescence. Eight years later, Yerkes' book, *The Dancing Mouse* (**132**) appeared, and that treatise dealt among other things with ontogenetic aspects of behavior.

In recent years the amount of attention paid to this subject has increased significantly, and the change is attributable to several different sources of stimulation. Some of the first systematic experiments dealing with behavioral development were generated by the problem of the relative importance of "maturation" and "practice" in the perfection of simple response patterns, such as the pecking behavior of young chicks (**117**) or the swimming behavior of larval amphibians (**16, 47**). Additional impetus was given to investigations in this field by the impact of Freudian theory (**38**), which led to various studies, such as those which limited food supply or feeding responses in young animals and then measured the effects upon certain types of behavior in adulthood. A third influence, slower to gain recognition in America, has derived from the observations and theories of Konrad Lorenz and other European biologists who examined the effects of early social stimulation upon the adult behavior of various species of birds.

The most recent increase of interest in the effects of early life experiences upon the behavior of adult animals is traceable to theories that

stress the importance of perceptual learning in infancy upon subsequent performance in tests of learning. A leader in this field is Hebb, whose *The Organization of Behavior* (**52**) has been directly or indirectly responsible for a number of experiments reported in psychological journals during the last two or three years.

Because of the rapidly growing interest in this area of comparative psychology, we have assembled and evaluated as much as possible of the relevant evidence. This seems worth doing not only because of its usefulness to students of animal behavior, but also because scientists interested primarily or exclusively in human psychology very frequently turn to the literature dealing with lower animals in connection with discussions of human development. The area covered is a diverse one; it touches and illuminates many fields of scientific investigation; its findings bear on a wide variety of problems ranging from plant parasitism to human personality; and its significance stretches from the genesis of sensory experience to the theory of evolution. Since the resulting diversity is very extensive, we have found it most feasible to organize our review in terms of the types of behavior which investigators have measured after modifying the animal's early experiences.

As the reader will discover, we have given the term "experience" a very broad meaning and used it to include a variety of factors ranging from the type of food consumed by larval insects to the characteristics of visual stimulation experienced by infant chimpanzees. This plan has been followed because of our conviction that there is no clear-cut line of division between the effects of early "psychological" experience and certain physiological antecedents of adult behavior. The development of behavior can be examined from either the psychological or the physiological viewpoint, but this is an arbitrary, methodological distinction made purely for the sake of convenience. Any analysis which is confined to one of these alternatives is bound to be incomplete and to a greater or lesser degree misleading.

Sensory Discrimination and Perception

Descriptions of the sensory and perceptual abilities of animals have very often implied the unwritten suggestion that these abilities are exclusively determined by innately organized neural mechanisms. Several recent studies tested this assumption by studying the effect of early sensory experience upon the sensory abilities of the adult.

Vision. Visually directed responses of laboratory rats are not modified to any marked degree if the animals are prevented from using their eyes during infancy. Animals of this species which have been reared in darkness proportionally adjust the force of jumping to a food platform at different distances, although during the first trials the accuracy of such jumps is inferior to that of normally reared animals (**75**). Rats reared in darkness are capable of size, brightness, and pattern discrimination after very little visual experience (**49, 50**), and correspond to normal animals in their ability to generalize from one visual stimulus to another on the basis of relative size or relative brightness (**50**).

Investigations of the importance of early visual stimulation in other subprimate vertebrates have shown more pronounced effects, and suggest the existence of marked differences

between species. It is reported, for example, that when a rabbit which had been reared in darkness was first brought into the light, it assumed abnormal bodily postures and bumped into obstacles as it moved about the environment (**40**). The eyeblink reflex in the dark-reared rabbit was normal, but the latency of pupillary response to light was retarded; it was five seconds in duration. After one month under normal illumination the visually controlled movements of this animal had improved to normal.

This result was not attributed to anatomical degeneration, since examination of the retinas and optic tracts of other rabbits reared in darkness revealed no structural abnormalities. Recent work, however, contradicts this earlier conclusion. Chemical analysis of the ganglion cell layers of retinas from rabbits reared for the first ten weeks in total darkness showed a complete absence of the pentose nucleoproteins present in normally reared animals (**13**). The cause is presumed to be the lack of adequate visual stimulation during the postnatal period of development. It is therefore possible that the visual deficit found in dark-reared animals is attributable to incomplete chemical development of the ganglion cells in the retina.

Pigeons whose eyelids had been sewn together before they had opened behaved abnormally when the lids were opened six weeks later (**88**). Like the rabbit reared in darkness, these birds tended to assume abnormal postures and apparently were unable to avoid obstacles on the basis of visual cues. Furthermore, they did not show the usual startle response when a hand or handkerchief was passed rapidly before their eyes. Some reflexes, including pupillary responses to light, were unaffected by the lack of previous visual stimulation, but optokinetic nystagmus was impaired. In general, posture, obstacle avoidance, and visual responses improved rapidly and were normal three days after the lids had been freed.

A similar, partial visual deprivation was produced by another experimenter (**118**), who encased the heads of ringdoves with translucent vinyl hoods when they were three days old, before their eyes had opened. This technique permitted diffuse uniform light to reach the retina, but prevented form perception. Two months later, some opacity was found in the vitreous humor of the eyes, but contrary to the previously cited study, there were no differences in the optokinetic reactions of the hood-reared doves and normally reared birds. No observations on obstacle avoidance or posture are reported, but when first unhooded and forced to fly, the ringdoves did so erratically and with definite errors in space orientation. For purposes of some future experiments, the experimenter cut windows in the hoods of the birds when they were two months old, so that only the lower half of the eye could be used, and then tested the birds on a form-discrimination problem. It was found that the hood-reared birds took significantly longer to learn a circle versus triangle discrimination than similarly tested controls, the conclusion being that early experience is a prerequisite to normal adult function in visual perception.

The deleterious effects of prolonged visual deprivation are not limited to mammals and birds. Eyed fishes taken into light from lightless subterranean cave pools repeatedly bump into obstacles when swimming about in a strange aquarium. They fail to

seize food as it drifts downward through the water, but instead seem to rely on chemical sensations as they grub about in the sand covering the tank floor. Visual control of swimming and feeding appears gradually and is only complete after several weeks under normal illumination. Fish that were reared in the laboratory in lightproof tanks for two and one-half years behaved like those captured in underground lakes and did not show normal visual responses until they had spent about one month in the light (**14**).

Most primates depend heavily upon visual cues, and it is not surprising that lack of visual stimulation in early life has pronounced effects upon subsequent behavior. Chimpanzees that were kept in the dark for the first 16 months of life were comparable to the pigeons described above in that they showed a marked pupillary response to increase in light intensity, but optokinetic reflexes were impaired and there was no blink when an object suddenly approached the eye (**105**). These apes exhibited head and eye movement to follow a point of light in a darkened room, but steady fixation and obstacle avoidance on the basis of visual cues were totally absent. In one case approximately 50 hours of visual experience elapsed before the first signs of visual learning (reaching for the nursing bottle) appeared (**105**). These results may have been due to physiological changes resulting from a lack of visual stimulation, since ophthalmoscopic examinations at a later date revealed evidence of severe retinal and optic nerve degeneration (**107**).

In a second experiment using chimpanzees, one animal was reared in total darkness to the age of seven months. A second was reared in the dark but exposed to diffuse light (no image formation possible) for one and one-half hours per day. A third ape was taken from the dark each day and allowed to spend one and one-half hours in a normally illuminated room (**106**). Tests begun when the animals were seven months old showed that the individual with full visual experience for 90 minutes per day behaved like a normal chimpanzee. In the partially deprived ape (exposed only to diffuse light), the blink response to objects approaching the eye appeared after six days of visual experience, while in the totally deprived ape (no light during the first part of the experiment), the blink reaction did not appear until 15 days. Steady fixation appeared in 13 days in the partially deprived, and 30 days in the totally deprived animal. Learning to avoid a conspicuous visual stimulus which was associated with electric shock took one or two trials in one day for normal apes, 13 days of two trials per day for the partially deprived individual, and 15 days of two trials per day for the chimpanzee reared in total darkness. Ophthalmoscopic examination revealed evidence of retinal degeneration in the totally deprived animal, but no ocular abnormalities were found in the other two animals (**107**).

The evidence is fragmentary and at points inconclusive, but it does suggest that in at least some species of fishes, birds, lower mammals, and primates absence of the normal amount of visual stimulation during the developmental period may result in inability to respond adaptively to visual cues when such cues first become available to the individual. The defects do not appear to be permanent but seem to disappear with in-

creasing experience in visually directed responses.

Touch and proprioception. There is some reason to believe that some other types of sensory deprivation in infancy may affect subsequent behavior. One anecdotal account describes a young chimpanzee whose paws were badly injured in infancy and which, as a consequence, never indulged in the frequent climbing behavior typical of this species. It is reported that after its paws had healed, this animal rarely exhibited any tendency to climb and showed signs of dizziness whenever it got more than a few feet above the ground (**53**).

A more recent experiment involved bandaging or encasing an infant chimpanzee's lower arms and legs in cardboard tubes and thus preventing the normal manipulative use of hands and feet (**92**). The restraining devices were in place, on the single subject, for 30 months, beginning with the fourth week of life. When the limbs were freed, the ape showed some forced grasping, which normally disappears during the first postnatal month. Walking and sitting postures were abnormal. Grooming behavior, so typical of the normal chimpanzee, was lacking, as was the almost universal tendency to cling to the human attendant when picked up.

When a normal chimpanzee is touched lightly at some point on the body, the usual response consists of promptly bringing one or both hands to the locus of stimulation. In the experimental animal these responses were either lacking or abnormally slow and inaccurate. Though visual discrimination in this animal was normal, the ape had great difficulty in learning to differentiate between tactile stimulation of the right and left hands when vision was excluded.

Feeding Behavior

Pecking responses in birds. As noted in our introduction, the controversy regarding the relative importance of maturation and experience in the shaping of behavior led to several studies of pecking behavior in chicks (**12, 25, 87, 95, 112**). These have been reviewed so often that they need not all be discussed here (see **80**). In general, it appears that birds which are prevented from pecking by being reared in darkness, and then tested for the accuracy of such behavior, tend to be somewhat inferior to normal controls at first, but to improve rapidly and equal fairly soon the performance of chicks that have not been deprived. The most interesting report from the point of view of the present discussion is the statement that dark-reared chicks, fed artificially from a spoon for two weeks after hatching and then placed in the light, never developed the pecking response. Put in a pen with an ample supply of grains and grit to peck at, "they all starved to death even in the midst of plenty" (**95**, p. 436).

As noted earlier in this review, some animals that have been reared in darkness are incapable of normal visuomotor behavior. It may be that part of the failure of dark-reared chicks to peck normally is due to sensory or perceptual deficiencies rather than to delayed motor development alone. Preventing the pecking response for long periods after birth has not been found to cause the disappearance of that response in other birds. Ringdoves, for example, were artificially fed from three days of age and prevented from pecking by being reared with translucent hoods over their heads (**118**). After

as long as two months, the hoods were removed, and the birds developed coordinated pecking at grain within 15 hours.

The visual stimuli capable of evoking the pecking response can be modified by early feeding experience. One group of chicks was reared for seven weeks in a cage which was illuminated exclusively from above, while in the cage of a second group the only light came through the translucent floor. Later the response to photographs of grains of corn was tested. In some pictures the corn was illuminated from above, in others from below. The chickens reacted selectively, pecking only at those photographs in which the lighting conditions corresponded to those under which real feeding had taken place (**55**).

A different approach to problems of pecking activity has been suggested by Levy's theory that there exists a pecking "need" or "instinct," and that birds continue to peck until this need is satisfied or satiated. To test this hypothesis, one large group of hens was raised in a cage having an elevated floor of wire mesh, while a control group was kept on a dirt floor (**77**). Chickens kept on wire pecked at their cage mates until many birds were partially denuded of feathers. Comparable behavior was rare among the control birds, which could peck the ground. When transferred to ground cages at six weeks of age, and mixed with control cases, the chickens reared on wire vigorously pecked at the backs of the fully feathered controls, but this behavior died out after two months. Another group of chicks was reared on wire floors for a longer period of time. It was found that the pecking of companions was progressively reduced and ceased after four months, but when transferred to the ground at seven months of age these birds again began to peck one another as vigorously as before.

Nursing behavior in mammals. Levy has suggested a comparable theory for the sucking responses of mammals. It is proposed that under normal conditions, a "need" to suck is satisfied as the infant sucks the mother's teat or breast, but if the need is not satiated during nursing, nonnutritional sucking behavior will appear. The occurrence of chronic thumb sucking in some bottle-fed babies and the reported ear licking and tail sucking of bucket-fed calves have been interpreted in this way (**76**). In an attempt to test the hypothesis, four puppies were separated from the bitch at 10 days of age and fed from nursing bottles thereafter (**76**). Two dogs drank from nipples with very small holes so that to obtain their milk ration they sucked an average of 80 minutes a day. The other two animals were fed from large-holed nipples and consumed the milk supply in an average of 13 minutes of nursing per day.

It is reported that the rapidly fed puppies exhibited sucking movements after feeding time and made sucking noises while asleep. In addition these dogs tended to suck their own bodies and those of other animals as well as to lick inanimate objects in the kennel. None of these characteristics appeared in the two animals that had the longer periods of nursing during infancy. A more recent study has investigated the possibility of producing this effect through sucking deprivation at an earlier age (**109**). Three puppies were partially deprived of sucking experience but adequately fed for the first 10 days of life and then returned to the mother. During the deprivation period the experimental animals showed a stronger

sucking pressure and more nonnutritional sucking than normal controls. These differences disappeared, however, when the dogs were returned to the bitch, and there was no persistence of the compensatory sucking such as had been reported in the earlier experiment.

Hoarding behavior in mammals. Laboratory rats do not normally hoard their food, as do many other rodents, but they can be induced to do so under appropriate conditions. The evidence is somewhat contradictory, but the results of some investigations indicate that early experience with food may have some effect upon hoarding in adulthood. According to the first study of this problem, rats reared on powdered food hoard fewer food pellets than other animals that have always eaten pellets; and subjecting rats to semistarvation in prepuberal life increases their tendency to hoard food in adulthood, even though the animals are satiated at the time of testing (**131**). Results of a subsequent experiment suggested that partial starvation for 15 days immediately after weaning produces increased hoarding in adulthood, but only if the hoarding tests are preceded by a period of food deprivation (**59**). In this same investigation it was found that partial starvation later in life, beginning 12 days after weaning, has no effect on food hoarding in adulthood. Attempts to reproduce these results in later experiments showed hoarding differences between rats starved in infancy and controls, but the differences were not as statistically reliable as those found previously (**60**).

A more recent attack upon this general problem involved study of the effects of infantile deprivation on the hoarding of water-soaked cotton as well as food (**79**). Albino rats of both sexes were divided into three groups. For 15 days after weaning, one group was partially deprived of food, and a second group of water. A control group was maintained on ad libitum food and water until adulthood. At 130 days of age, all animals were placed in special cages where hoarding tests were conducted, both before and after a period of deprivation. The food-deprived males were found to hoard significantly more food pellets than controls after adult deprivation, but no significant differences between deprived and control animals were found among the females, or in the water-hoarding tests for either sex. The picture presented by such results is of a more complicated problem than was at first supposed.

Positive results in studies of this type may be due to the prior acquisition of pellet-seizing and pellet-carrying responses, as well as to an increased rate of eating in the experimental animals (**82**). This hypothesis has been tested, and it was found that rats deprived severely in infancy had a faster eating rate as adults (**83**). Presumably, such rats would need less time to eat in the hoarding tests and would therefore have more time to hoard extra pellets.

The positive reports of increased hoarding consequent to infant feeding frustration have been rather widely cited as bearing upon psychiatric theory (**57, 104**). In view of the lack of clear-cut confirmation by subsequent experimenters, final judgment probably should be reserved.

Reproduction

Among the most species-specific behavior patterns in any particular species are those having to do with reproduction—courtship, fertilization, nest building, parturition, and the

care of the young. For a long time, these often unique stimulus-response organizations were classified as "instincts" and were thought to be exclusively determined by genetic constitution. Evidence which shows that the occurrence of these responses is heavily dependent upon other factors, especially the early experience of the animal, has recently been gathered from many widely divergent invertebrate and vertebrate species. It is worth stressing at this point that early experience appears to influence the kinds of stimuli that will later evoke reproductive activities, whereas the actual responses are less subject to modification.

Egg-laying behavior in insects. Several studies have shown that the particular chemical stimuli which call forth the egg-laying responses of gravid females of several insect species are determined in part by the type of food which the insect has eaten or been exposed to during the larval stage of life. The female sawfly normally deposits her eggs on the leaf of a particular species of willow tree, but if forced to do so she will oviposit on a different kind of willow. In an early experiment, successive generations of females were forced to deposit their eggs on an unpreferred species, and, after four years, the descendents showed a preference for the new type of host plant (**46**). However, the mortality in this study was high, and the results could possibly be explained on the basis of genetic selection.

More carefully controlled studies have employed a choice method in which the insect's tendency to approach different hosts is tested in a Y-shaped tube. Thorpe and Jones (**128**) found that all adult females of the ichneumon fly are attracted to the odor of meal-moth larvae, which are the normal hosts. But insects that had been reared on wax-moth larvae during their own larval period were also attracted to the odor of larval wax moths, while insects reared on normal hosts were not. Immediately after they had hatched, normally reared females were brought into contact with meal-moth larvae, and as a result of this exposure they showed temporary attraction to the immature meal moths; but the attraction was lost if the ichneumon females were isolated for 10 days (**123**). Similar changes in adult preferences produced by varying the type of food consumed in the larval stage have been reported for fruit flies (**124**) and vinegar flies (**26**).

Thorpe (**124**) regards this type of modification of behavior in insects as a conditioned response and comparable to imprinting phenomena in birds, which will be discussed later in this review. Whatever the explanation, these experiments do demonstrate: (*a*) that adult olfactory preferences in some invertebrates can be affected by early environment, and (*b*) that this type of effect can in turn influence egg-laying behavior in these particular species. It is important to add that this conditioning effect does not occur in all insects. When females of the Colorado beetle were reared as larvae on a foreign type of host plant, they did not oviposit on the new plant (McIndoo, as cited in Thorpe, **124**), and descendents of a strain of chalcid wasps which had been confined exclusively to a foreign host for 260 generations still preferred the ancestral host (**111**).

Sexual selection in fish and birds. The reproductive behavior of some fishes and of many birds is characterized by the comparative importance of elaborate patterns of display and courtship which are usually an indis-

pensable preamble to the mating act itself. The overt responses involved do not appear to be modified by prior experience, but in some cases the stimuli that elicit display and courtship have been found to be markedly dependent upon certain kinds of early experience.

Although adult male cichlid fishes apparently show courtship and mating responses only toward members of the opposite sex, males that were initially reared in isolation attempted to mate indiscriminately with both males and females (**93**).

Passenger pigeons reared by foster ringdove parents are said to have failed to mate with their own species as adults, but to have attempted sexual union only with birds of the same species as their foster parents (**23**). Other ornithologists have noted similar interspecies "sexual fixations." Turtledoves, domestic and wood pigeons, and magpies have been reported to pair with individuals of other species if they are reared with that species (**41**). Unfortunately, the lack of experimental controls renders the interpretation of such observations difficult, but the results suggest that in some species of birds, individuals tend to mate with the species with which they have been associated in infancy.

Comparable effects upon adult mating preferences are sometimes found in birds that have been reared by human beings. Three male, blond ringdoves were reared singly by Craig (**24**). When they were first put into a cage with a female dove, these males directed their courtship and coital responses to the hand of their caretaker or to inanimate objects in the cage. Eventually, normal mating between the birds occurred. It has been reported that a domestic turkey cock, which had been reared by human males, responded to men with the usual courtship and mating activities, whereas women elicited the fighting and retreating reactions normally reserved for rivals of the same sex (**101**). This cock never displayed to turkey hens, but would tread them or any similarly sized object if he were forcibly placed in the mating position.

Mating behavior in mammals. No interspecies mating preferences due to special conditions of rearing have been reported for mammals, but other effects of early experience on sexual activity have been described. One early study of male rats led to the conclusion that prepuberal segregation of males increases the tendency of an animal to cross an electric grid when a male is on the opposite side and decreases the tendency to cross to a female (**65**). However, the interpretation of results obtained with the obstruction method is difficult, and the unsystematic procedure of this experiment places its results in serious doubt. Other studies (**7**) do not support the suggestion that preference for a masculine sex partner is influenced by the conditions of rearing. Rearing rats in isolation has no effect on the pattern of the copulatory response itself either in males (**7**) or in females (**122**), but it does tend to increase the proportion of males that copulate in comparison with other groups that have been reared with females or with other males (**7**).

An effect of prepuberal experience on adult sexual activity in the male rat has recently been reported (**66**). Male rats were reared in individual cages from infancy, but periodically throughout the prepuberal life phase they were exposed to age mates of either sex in standardized ten-minute tests. When tested with sexually receptive females in adulthood, these

males displayed some copulatory responses, but also indulged in a great deal of the wrestling and play behavior that is characteristic of the immature rat. Ejaculation in the male rat normally follows a series of closely spaced intromissions, but in the experimental males, the frequency of intromission was greatly reduced by the interpolation of play reactions, and ejaculatory responses were therefore infrequent.

Parental responses in mammals. Care of the young depends in some instances on experience gained before sexual maturity. Early experiments revealed that primiparous rats, confined in individual cages up to the time of parturition, made use of available nesting material and cleaned and cared for their young in a biologically adequate fashion although they had never seen nests built or delivered litters (**6**). It appears, however, that nest building is in some fashion related to earlier experience in the manipulation of objects in the environment. Female rats that were reared in cages containing nothing which could be picked up or transported are reported to have built no nests at parturition, even though the appropriate type of materials was available (**108**).

In an as yet unpublished study, female rats were reared with rubber "Elizabethan ruffs" around their necks (Birch as cited in Riess, **108**). This technique prevented the female from washing herself or from having any mouth contact with her own genitalia. The ruffs were removed shortly before parturition, and it is reported that the experimental females failed to clean or to nurse their young.

Effects of captivity or domestication upon reproductive behavior. The effects of rearing animals in captivity sometimes include the appearance of sexual maturity at an earlier age than has been observed under natural conditions, as well as extension of the breeding periods in adulthood. This occurs in a variety of mammalian species including the ibex, wild boar, American bison, antelope, chamois, and others (**53**). In one controlled study, the offspring of wild grey rats (*Norwegicus*) were bred for 25 generations in the laboratory. In the course of this time, the length of fertility in relation to the life span was doubled, although average litter size did not change (**71**). Some mammals, however, such as the polar bear (**120**), show no alteration in the length of the breeding season under captivity, and failure of normal sexual development has been reported in three gorillas reared in captivity since infancy, and also in the cheetah and Malayan bear (**53**). Whether these effects have some physical cause, dietary or otherwise, or are due to early experience is not known.

Gregarious and Filial Behavior

Some species of animals maintain colonies, schools, flocks, or herds throughout the year. In other species, formation of social groups is limited to the breeding season or to the time during which the mother and young must remain in close association. In both instances, study of the influence of early experience on social behavior has yielded important results.

Species recognition in insects and fish. Among invertebrates, such aggregations as have been studied seem to depend on some single specific stimulus. For example, each colony of ants, even within the same species, has a unique odor. Members of a colony recognize each other by means of this cue, and an ant contaminated with the odor of a different colony is usually attacked and killed (**9**). It is

possible that this olfactory basis of social organization is partly determined by early experience, as is host preference in some parasitic insects. If normally antagonistic species of ants are placed together within 12 hours after emerging from the pupae, there is no aggressive behavior between them, and a permanent mixed colony can be established (**32**). The effects of this early "conditioning" are apparently permanent, for temporary separation of a mixed colony of two species after a month of association did not decrease their compatibility. Even after seven months of separation, ants that were originally reared in a mixed colony did not attack those of the same odor with which they had been reared, though all other "foreign" ants were promptly attacked and killed (**33**).

Visual stimuli are commonly involved in the social interaction of several kinds of fishes, and in some species responsiveness to a given visual stimulus is influenced by early experience. Adult jewel fish of one type show a blue-black color of the ventral surface during the brooding period. The fry of this species normally have a positive response to blue-black objects and are unresponsive to red. But young fish that have been reared by foster parents with brilliant red underbellies show a reduced response to blue-black, and a positive reaction to red (**94**). In other words, the sensory basis for filial grouping in these fishes is partly dependent on very early experience.

Gregariousness in insects. The color, morphology, and aggregation behavior of solitary and migratory locusts differ quite markedly, and at one time they were classified as separate species. It is now known that these striking differences are "phases" of the same species (**129**), and that the differentiating characteristics are directly related to certain conditions existing during the insects' development. In an early experiment it was found that progeny of the solitary locust could be transformed into the migratory type by rearing them under crowded conditions, whereas individuals reared in isolation never become migratory (**31**). This relationship of early environmental conditions and adult behavior in the locust has been confirmed in field observations (**70, 130**).

A well-designed experimental analysis of social behavior in locusts has recently been performed and further strengthens the above conclusions (**28**). Groups of locusts in the first instar (i.e., from hatching to the first metamorphosis) were reared under crowded conditions, and others were reared in individual jars. When the experimental animals had metamorphosed, and were therefore in the second instar, aggregative tendencies were measured by placing groups of them in a ring-shaped cage, the floor of which was marked off in equal-size segments. At intervals after the beginning of the test, the numbers of insects in each cage segment were counted. By comparing the frequency distribution of segments containing various numbers of individuals with a Poisson series it was determined that locusts which had been reared in groups aggregated at a level well above chance, whereas insects reared in isolation were significantly less gregarious.

When the formerly isolated locusts were kept under crowded conditions for 24 hours, there was a considerable increase in gregarious behavior as measured by the same test, and after four days of group living, their social behavior was identical with animals reared in groups since hatching. In contrast, the social behavior of locusts reared for the first instar

in crowded conditions was not appreciably affected by subsequent isolation. This suggests that in this species the tendency to aggregate, once it has been acquired in infancy, is irreversible.

Gregariousness in birds. The effects of early experience on social behavior seem to be much more complex and extensive in most birds than in fishes. Lloyd Morgan observed that domestic chicks reared in an incubator are relatively indifferent to the hen and her flock (**86**), and recent experiments indicate that hand-reared chicks respond very slowly, if at all, to the food-call clucking of the hen (**20**). Mallard and Muscovy ducklings reared in an incubator do not show the normal selective response to adults of their own species. Qualitative measures of the tendency of isolated chicks to move toward another chick in a runway revealed that the approach was much slower than normal, with frequent pecking at the second bird. When they were placed with the flock, such chicks tended to keep apart from the group (**19**). These antisocial effects are maximized by rearing chicks in complete isolation away from any animal or human being. Chicks so reared are said to have developed a persistent and "rage-like" restlessness as well as unusual stereotyped behavior patterns, and to have reacted in a "fearful" fashion when placed with the flock (Bruckner, as cited in Katz, **68**).

There is some reason to doubt that rearing chicks in isolation completely abolishes all positive responses to others of their kind. Chicks have been reared in isolation to the fourth day after hatching, and then tested for their choice between a stimulus compartment containing chicks and one containing mice. They spent more time in front of the former (**97**). Interestingly enough, birds that had been raised in the flock showed no such preference.

"Imprinting" in birds. In studying reproductive behavior of birds, we noted several instances in which the choice of sex object by the adult bird was influenced by the species with which the individual had been associated in infancy. That other social responses can become "fixated" in this way, especially toward human beings, has long been known. It is said to be true, for example, of the rhea bird (**58**). If eggs of the European greylag goose are hatched in an incubator and the young are exposed to a human being just after hatching, the young birds respond in filial fashion to the human but not to adults of their own species (Heinroth as cited in Lorenz, **78**). It is further claimed that incubator-hatched geese can be reared by mother geese only if they are placed in a sack immediately upon removal from the incubator, so that premature exposure to humans is prevented. To describe the process responsible for these phenomena, the German ornithologist, Konrad Lorenz, has coined the term *Prägung*, translated as "imprinting." Imprinting takes place when the first stimulus to evoke a given "instinctive" reaction becomes, with extreme rapidity, the only stimulus that henceforth can evoke that reaction. According to Lorenz, imprinting has two properties which distinguish it from other types of learning. First, it can only take place during a specific and brief portion of the life span while the organism is in a critical physiological stage of development; and second, the results of imprinting are irreversible throughout the life of the individual.

Lorenz confirmed Heinroth's observations on the greylag goose. Newly hatched goslings were exposed

to and allowed to follow the experimenter and were prevented from seeing, hearing, or being in contact with adult geese until the tendency to follow the human foster parent had been firmly established. Thereafter, they tended to follow and remain close to Lorenz and showed no positive social responses to adult geese (**78**). There is evidence to show that imprinting occurs in some other birds, especially in those ground-nesting species whose young are capable of locomotion and self-feeding as soon as they are hatched. Young chickens, ducks, or turkeys have been reared by foster parents belonging to one of the two other species and under such conditions have been found to form cross-species filial attachments (**102**).

A number of independent studies combine to indicate that susceptibility to imprinting is a function of the age of the young bird. The coot is most easily affected by imprinting during the first 8 hours after hatching, and coots that are 24 hours old cannot be imprinted (**1**). The most thorough study of filial imprinting has been made on ducklings (**29**). The young birds (tufted ducks, eiders, and shovellers) were hatched in an incubator under opaque hoods. At various durations after hatching, the hoods were removed, and the ducklings were allowed to follow the retreating experimenter. It was found that for all three species the following-reaction could be evoked, and imprinting established most easily in the first 6 hours and, as in the coot, could not be obtained if 24 hours had elapsed since hatching.

Not all birds followed the experimenter without his also making auditory signals. Such sounds increased the probability of evoking the following-reaction and at least in the tufted duck, the shoveller, and the mallard seemed to have a stronger releasing effect than visual stimuli. Birds that had been imprinted subsequently followed the experimenter whether he walked, swam, or rowed a boat, and paid no attention to adult ducks. After three weeks, the following-reaction to humans gradually diminished.

In tree-nesting birds, whose young have a slower behavioral development, the formation of social attachments takes a somewhat different course. The initial phase of imprinting to a parental object is believed to occur as it does in ground-nesting birds, but the results do not become irreversible until some time later. For example, jackdaws taken from the nest at 14 days of age were found to be imprinted on their natural parents, but after separation from adult jackdaws and continued association with humans from this time on, the birds' positive social responses could be shifted to human beings (**78**). After 20 days, when the young jackdaws are fledged, such shifts in social responsiveness cannot occur. Those that have been with adult birds cannot be imprinted to humans, and those that have earlier been imprinted to humans cannot be induced to react normally to their own kind. These preliminary findings on one insessorial species are of interest, but they require a more thorough conceptual and experimental analysis before they can be generally accepted.

There are some avian species in which parental imprinting to humans does not occur. For example, some wading birds, such as curlews and great godwits, show the escape reaction to man immediately after hatching in an incubator, and hence imprinting on man is impossible (**78**).

It is interesting to consider the possible role of imprinting in the evolution of species. It seems highly probable that it operates as a social

and reproductive isolating mechanism in the case of sympatric species. That it may also play a role in fostering the geographical isolation of some allopatric species should be of interest to ecologists. Thorpe has pointed out that the significance of imprinting may not be confined to the formation of bonds with other living creatures, but may apply also to bonds with inanimate aspects of the environment (**126**). Evidence for this is scant, but it is known that acoustic signals (**29**) or boxes and footballs (**102, 127**) can be used as imprintable stimuli for various birds. Validation of this hypothesis might aid in understanding the formation of ecological races. For example, peregrine falcons nest in cliffs in one region and in trees in another, but in periods of overpopulation, the cliff-district birds unadaptively attempt to nest in unsuitable cliffs rather than switch to trees like falcons of other districts (**56**). This may be due to imprinting of the young falcons to cliff nests.

Song acquisition in birds. The songs of birds are an important mechanism in their social organization, especially in the breeding season, and it is widely believed that in some cases these songs are influenced by auditory experience acquired early in life. For some species, such as the rhea bird (**100**) and roller canary (**85**) no such influence is found, but if young common canaries are allowed to hear the recorded song of the nightingale they develop a song intermediate between the normal canary and nightingale songs (**61**). In another study, bluebirds, robins, woodthrushes, orioles, bluejays, and redwinged blackbirds were reared together in a large room. At maturity, none of these birds sang the normal song of its species (**115**). Similar modifications of the adult song resulting from conditions of rearing have been reported for other species including bobolinks, grosbeaks (**116**), sparrows (**21**), whitethroats, tree pipits, meadow pipits, greenfinches, chaffinches, and yellow buntings (**54**).

Song acquisition in birds has been considered as a special type of imprinting (**127**). This interpretation is supported by the report that some birds, like the chaffinch, never acquire the normal song if they have developed an abnormal one in early life (**43**). That this also may be a case of an isolating mechanism directly related to early experience has been suggested by Mayr (**84**) in his discussion of the song races of chaffinches about Stuttgart. A male chaffinch, who had by some chance association with other birds learned a different song in infancy, would be expected to employ this song in courtship. If he were successfully mated, the progeny would then be exposed to the special song, acquire it, and use it in their courtship later, and in this manner a new species or subspecies might develop. Thus it is seen that in birds early experience can exert a marked influence upon the future social life of the individual, and also perhaps upon the future of the species.

Social behavior in mammals. Studies of the social effects of early experience in mammals are relatively few. Naturalistic observations reveal that lambs of wild sheep show something like the imprinting process in birds. When captured and reared by humans, they do not later rejoin the herd, but follow and remain with human beings (**89**). Domestic lambs that have been reared on a bottle exhibit similar behavior. When turned to pasture with the flock, they show difficulties in adjusting to other sheep and tend to graze by themselves (**112**).

Scott and Marston (**113, 114**) have

outlined a theory to the effect that the development of young puppies is divisible into several sequential periods, certain of which are "critical" in the sense that during these relatively brief phases the young dog forms habits or develops tendencies which have profound effects upon social behavior in later life.

There is no experimental evidence to establish the point, but it may be noted that the parturitional behavior of certain wild ungulates is such as to allow maximal opportunity for newborn young to form social bonds to the mother within a very short time after birth. Female red deer and American elk associate in herds throughout most of the year, but just before calving, the pregnant female leaves the group and seeks seclusion. For several days after parturition the young remain isolated and hidden, and are visited periodically by the mother. Then both the female and her offspring rejoin the herd, and thereafter the calf's following and suckling responses are directed solely to its own mother, while she in turn permits only her own young to remain close to her and to nurse (**2**, **27**). The possibility suggests itself that during the brief period when the calf's social contact is restricted to its mother, imprinting or some similar process takes place.

The chimpanzee is a highly social animal and may be considerably disturbed by temporary isolation (**73**, **91**). The object of its social impulses appears to be influenced by experience in infancy, just as it is in many birds and in some subprimate mammals. Several observers (**48**, **63**, **69**) who have reared infant chimpanzees in their homes have noted how easily at this age the social reactions of these animals can be transferred to human adults. One such chimpanzee was later returned to the laboratory and caged with others of its own species. For several months he did not mix with other apes, but looked "longingly" at human beings who were visible to him beyond the cage (Finch, as cited in Nissen, **91**).

Emotion and Temperament

Wildness and timidity. To the human observer, animals in nature display characteristic behavioral tendencies that are often described as wildness, timidity, or aggressiveness. Some of the laboratory studies cited in this review indicate that certain emotional responses in animals are often directly related to experiences acquired early in life. In birds, we have seen how rearing in isolation produces unusual emotional behavior (Bruchner, as cited in Katz, **68**), fearfulness when placed with the flock, and a tendency to peck at other birds (**19**). Another study involved rearing domestic chicks in small wooden boxes inside the brooder where other chicks could be heard and their shadows seen. Even this partial isolation resulted in fighting behavior when each of these birds was paired with a normal chick at seven weeks of age. Fear reactions and "bewilderment" when placed with the flock were also reported (**15**).

"Wildness" involves both genetic and experiential factors. Among the latter, one significant natural variable for some species seems to be the temperament of the mother. Black ducks and mallard ducks are equally wild in nature, but the mallard's wildness is readily modifiable when the birds are hatched and reared by tame foster parents (**98**). Wild herring gulls that have been reared by the somewhat tamer common tern do not display the usual herring gull's timidity toward man (Kuhleman, as cited in Thorpe, **127**).

The tameness of the mother seems

to be an important factor in the wildness of some small mammals. Rats reared by tame females are more docile toward humans than those reared by wild mothers (**103**), though the taming effect is said to be cancelled out if a wild father remains in the same cage with the family (**110**). In another study it was found that descendents of wild gray rats reared in captivity for 25 generations lost their "high nervous tension and fear of man," although in this instance there was no control of the possible effects of selective breeding in successive generations (**71**). One commonly recognized early-experience factor affecting wildness is association with human beings. Lion cubs taken from the mother and reared by hand are tamer than those nursed by the lioness (**22**).

It has been suggested that severe trauma in infancy can affect emotional reactivity in later life. Young mice from a strain that is highly susceptible to audiogenic seizures when stimulated with the sound of an electric bell were exposed to this stimulus from the fourth day to the seventh day of age. No seizures resulted in these infant animals. However, when they were placed in the same situation at a later age, but not given an auditory stimulus, the "traumatized" mice displayed a higher frequency of eliminative responses than did litter-mate controls (**44**). These findings were originally interpreted as indicating a general increase in "emotional instability," but the test for emotionality was made in the same environmental setting where traumatization had previously been attempted. It is therefore impossible to rule out the alternative explanation that the results were due to direct and specific conditioning. In a similar investigation rats were used as subjects. Infant animals were exposed to a variety of supposedly traumatic stimuli and tested for emotionality in adulthood. No difference was found between experimental and control groups in an open field test or in susceptibility to audiogenic seizures (**42**).

In a study of emotionality in dogs, three Scottish terriers were raised as pets with a great deal of human handling, and another three were reared in restricted laboratory cages with a minimum of human contact. As adults, the cage-reared animals differed from the others in that they displayed "freezing" behavior in an unfamiliar environment, hugging the floor and staring forward, as well as a behavioral "anesthesia" to a hypodermic needle (**18**).

Aggressiveness. As noted earlier, chicks reared in isolation tend to peck other chicks more than do group-reared birds, and the fighting behavior of adult mammals of some species is markedly influenced by certain types of infant experience. Hungry infant mice were trained to fight over food shortly after weaning. As adults, these animals fought over food even when not hungry. Littermates, raised without competitive experience, did not show this behavior except under conditions of starvation (**36**).

Early defeats in a fighting situation have been shown to influence aggressiveness in later life. Mice of different ages were exposed to and attacked by a second mouse that had been previously trained to fight. The results showed that the younger the mouse was at the time of its initial defeat, the greater was the perseveration of nonaggressive, withdrawal responses in a fighting situation in adulthood (**67**). In experiments of this type, it is difficult to conclude whether the results are due to the age variable per se, or to the possibility

that a young animal would experience a greater amount of trauma than an older animal.

Learning

Many psychologists appear to conceive of learning ability in animals as genetically determined and relatively unmodifiable, but recent findings indicate the untenability of this thesis. Harlow (**45**) and his co-workers have demonstrated the development of "learning sets" in primates, wherein an animal "learns how to learn," and other studies have shown that stimulus-response learning in rats is significantly facilitated when they have previously learned quite different responses to the training stimuli (**64, 74**).

Results of a second group of experiments indicate that an animal's performance on special "intelligence tests" is directly related to certain kinds of experience in infancy. In one way or another, most of these experiments are outgrowths of a theory proposed by Hebb (**52**). In brief, this theory predicts that animals that have had a large amount of perceptual experience early in life will prove better learners than others deprived of such experience. Further, it is predicted that the magnitude of this facilitative effect is, within limits, inversely related to the age at which the perceptual experience is gained.

Evidence for this view has been obtained largely by varying the environment in which the animal matured. Rats reared from 21 to 51 days of age in a small room and not in cages were found to be superior in maze learning to other animals that had been reared in small cages or in "squeeze boxes" (**11**). In a more carefully controlled study, one group of rats was reared in a large cage, and members of a second group were reared individually in mesh cages which were periodically moved from one position to another in this large cage. A third group was kept in enclosed activity wheels, and a fourth group in enclosed stove-pipe cages. Rats in the first two groups performed significantly better on the Hebb-Williams maze than did the latter two groups. It was concluded that the visual perception in a spacious environment during early life facilitates adult learning ability (**62**). A further study confirmed the preceding results and added the fact that having early access to pieces of apparatus somewhat similar to parts of the Hebb-Williams maze further increased learning scores on that particular test (**35**).

Rats reared as pets in the home also show definite superiority in certain learning situations when compared with animals reared in laboratory cages (**51**). In this connection it should be noted that the mere handling of rats from infancy improves performance on a T maze in adulthood (**8**), and it is possible that the difference between the home-reared and laboratory-reared animals might be due to this factor.

The age at which the extended environment is experienced is an important factor. Rats reared for a period of time early in life in a large space are clearly superior on the Hebb-Williams maze to animals that are first kept in small cages and then put into the more favorable environment at a later age (**62**). Supporting the same conclusion in another way is the fact that rats blinded early in life are inferior in maze performance to rats blinded at a later age (**51, 62**).

One possible explanation for these differences may be that rats reared in a more spacious and heterogeneous environment develop a superior ability to use distance orientation cues in learning. This hypothesis is in accord

with the finding that being raised in a large cage does not improve the rat's ability to learn a simple alley maze (**62**), or a visual discrimination problem (**11**). Presumably distance cues are less important in such tests than in more complex measures of the Hebb-Williams type. Positive evidence for the hypothesis is seen in the fact that rotation of the Hebb-Williams maze interferes much more seriously with the performance of rats reared in large than those reared in small cages (**35, 62**).

Exploratory tendencies may also be involved in the differences under consideration. One of the first studies of this problem showed that the tendency of rats to explore a novel environment in adulthood is increased if the animals have been reared in large instead of small cages (**96**).

A few experiments have been conducted to extend some of these findings to other species. For example, dogs reared as household pets are described as superior in learning ability to others raised in laboratory cages (**18**). However, it is impossible to estimate the degree to which the marked emotional differences between the two groups of dogs, as discussed earlier in this review, may have contributed to differences in test performance.

A quite different aspect of infant experience that affects an animal's learning ability in adulthood is the amount of food available. Rats deprived of food in infancy, and thus morphologically stunted, show higher activity scores as adults, and take less time to learn a maze for a food reward than do normally reared animals (**3, 4**). This finding holds true for mice as well (**72**). A later study showed that these results may have been due to the fact that the physiological condition of inanition continued to the time of learning in the experimental animals, rather than to other effects of early experience (**10**). A perceptual factor may also be involved. Rats which have been deprived of food and water in infancy surpass normal animals in learning the locations of food and water in a maze even though they are satiated at the time of testing (**17**).

The studies of Harlow (**45**) on primate learning have shown how the learning performance of monkeys and chimpanzees may be increased by prolonged experience with various aspects of the problems presented. According to Hebb's theory, if this type of experience were gained in infancy it would have an even more marked effect on adult learning capacity, but very few such experiments have been conducted with primates. In one study a home-reared chimpanzee was compared on four simple problems with a laboratory-reared chimpanzee, and with four human children the same age as the animal. The home-reared ape and the children did equally well but the laboratory chimpanzee could only solve one out of the four problems. However, since the solution required imitation of the human experimenter, a requisite which the laboratory-reared animal had had no opportunity to acquire, the interpretation of these results is ambiguous (**48**).

Discussion

Reviewing the studies in the foregoing pages has left the authors with two basic convictions. The first is that a fuller understanding of the effects of early experience upon subsequent behavior is fundamentally important for a science of psychology and for the broader field of animal biology. A second and equally strong impression is that much if not most of the presently available evidence bearing upon this problem is equivocal

and of undetermined reliability.

Many of the studies cited in this review are primarily of a naturalistic character and some of them verge on the anecdotal. Approximately 10 per cent of the reports contain conclusions based upon the behavior of a single individual. Many of the recent laboratory investigations reveal weaknesses in experimental design, and some of them are marked by lack of sufficient skepticism in theoretical interpretation. We have noted at various points the way in which variables have been confounded without recognition of the fact. Thus, the effects of special conditions of rearing upon intellectual performance have been described in some cases without acknowledgement of the additional effects upon emotional stability which might well influence performance in learning tests. The results of preventing a particular response have been described without considering the confounding effects of deprivation in sensory experience. Natural selection in studies involving successive generations, speed of eating in measures of hoarding, and other variables have not always been subjected to control in the experimental design.

It must be admitted that there are instances in this area where independent control of all the relevant variables is difficult, if not actually impossible. This is especially true with respect to the age variable itself in studies designed to measure the later effects of experimental treatments at different age levels in infancy. Anatomical differences, nutritional requirements, sensory sensitivity, motor development, and previous experience are closely interwoven variables with age, and cannot usually be controlled independently of each other. Any effect ascribed to the different ages at which a treatment is given may be due, not to chronological primacy by itself, but to differences in any one of the above variables or combination of them. This dilemma does not detract from the value of studying such problems. Rather, it means two things: first, that there are stages in the history of every science when investigators are, for a time, forced by the limitations of their knowledge to study the interaction of variables, rather than isolated ones; and second, that such interactions should be seen and labeled as such whenever theoretical conclusions are made.

Because the general thesis that early experience does affect later behavior is so often taken for granted and assumed without further elucidation, it may be clarifying to conclude with some consideration of the basic ideas underlying this general type of research. In what ways does early experience influence later behavior? Broadly speaking, the evidence we have surveyed suggests three answers to this question.

Persistence in adult behavior of habits formed in early life. The persistence of acquired behavior through time is, of course, a general phenomenon throughout the life of an animal, but there are several reasons why habits acquired early in life might be especially persistent. When a very young animal learns a response to a stimulus situation, the occurrence of that response each time the situation presents itself may prevent the acquisition of other types of behavior which could compete with the original one. This appears to have been the case in some of the studies on pecking in chicks, in which the acquisition of an unnatural eating response to a spoonful of food prevented the development of the normal pecking reaction (**96**). The same explanation seems applicable to the finding that male rats which, in infancy, have formed habits of respond-

ing playfully to receptive females, never display completely normal sexual reactions after they become mature (66).

An additional reason why habits learned early in life are sometimes retained better than those which are acquired later involves differences in motivation with age. It is possible that certain physiological needs and their corresponding "drives" are more intense in infancy than in adulthood. Younger rats, for example, will make many more grid crossings to food than older rats tested after the same duration of food deprivation (81). Rats starved just after weaning are therefore probably hungrier and have a more intense experience than rats starved at a later period. As a result, the effects of early experience might be expected to persist because of the intensity of the motivation under which the experience originally occurred, while the effects of later experience would not persist. It is conceivable that Hunt's first results showing the greater effect of early than late deprivation of food on adult hoarding of food (59) can be interpreted in this way. A similar case could be made for the more lasting effects of early trauma. Because of the young animal's unfamiliarity with its environment, and its lack of a learned repertoire of fear-reducing responses, fear in infancy is probably of a greater intensity than similarly produced fear later in life. It is known, for example, that incidence of seizure reactions to air blasts is a decreasing function of age in rats (30, 34). In the same manner, defeat in a fight, as occurred in Kahn's study of mice (67), may be a more intense experience for a young animal than for an older one; and for this reason, its traumatic effects might last longer and have a greater influence on subsequent behavior.

Early perceptual learning affecting adult behavior. Quite distinct from the theory of retention in adulthood of the same overt responses learned in infancy is the hypothesis that certain types of early experience influence later behavior by structuring the individual's perceptual capacities. Practically all the studies we have cited in the fields of sensation and learning are based upon this view. Mention has already been made of Hebb's analysis of the development of behavior. Early learning is conceived to consist largely of the establishment of "perceptual elements" ("phase sequences," neurologically) which serve as the basis of learning later in life (52). It is far too early to evaluate the explanatory value of this theory, but several studies have shown that absence of normal sensory or motor experience in infancy can severely restrict the reactive abilities of adult animals.

Critical periods in development. A third idea which underlies many researches involves the concept of specific stages in ontogeny during which certain types of behavior normally are shaped and molded for life. This hypothesis may have been suggested by certain findings in experimental embryology. If at a certain stage in the development of the frog embryo, part of the presumptive belly wall is transplanted to the region where the nervous system is later to develop, the transplanted tissue develops into a portion of the neural tube—the future of the cells thus being determined in part by their environment, a process which Spemann (121) called induction. At a later stage, however, the cells lose their susceptibility to such environmental influence. Similarly, there may be relatively brief stages in the behavioral development

of animals during which the future of certain aspects of behavior is strongly affected by contemporary environmental influences that have no such effect at other points along the life line.

Applying this theoretical viewpoint to the behavioral development of birds, Lorenz (**78**) concluded that there are specific and restricted periods during which the stimuli which will thereafter evoke certain instinctive responses are permanently determined. After the critical period has passed, the environment cannot alter the nature of the effective stimulus. Studies on imprinting suggest that this may be true for the filial responses of ground-nesting birds, in which case the critical period is restricted to the first 24 hours after hatching (**1**, **29**).

More recently, the idea of critical periods has been applied to dogs. Scott and Marston (**114**) divide the development of the puppy into sequential periods according to the kinds of behavior that are possible. First is the neonatal period, lasting approximately ten days, when the animal is both blind and deaf, and therefore somewhat isolated from the environment. A transition period follows to three weeks of age, at which point conditioned responses are said to become possible fairly suddenly (**39**). Then comes a socialization phase, lasting to ten weeks of age. This is described as a critical period affecting the future social adjustment of the individual (**113**). It is followed by the juvenile period, and, after sexual maturity, the adult stage.

There are several obvious similarities between the hypothesized critical periods in animals and various theories for the development of human behavior. For example, Freud's concept of discrete stages in psychosexual development and the lasting effects of fixation at one level or another calls to mind Levy's demonstration that interference with normal oral responses in a period when they predominate in an animal's behavior may influence later behavior of the same kind. And Murphy's use of the concept of canalization (**90**), which is regarded as an irreversible learning process whereby some needs come to be satisfied only or predominately by certain, specific stimuli, bears a resemblance to Lorenz's theory of imprinting. Descriptions of the developmental stages in puppies are somewhat reminiscent of Piaget's account of the progressive development of social behavior in young children (**99**). It is tempting to suggest the existence of similarities in underlying mechanisms, but this would be unwarranted on the basis of present knowledge.

In listing three possible answers to the question of how early experience can affect later behavior, we mean to imply neither that they are mutually independent, nor that they are together exhaustive. Rather they appear as currents of thought along which research in this area has progressed.

In conclusion, it can be said that the longitudinal approach to the study of animal life is obviously more beset with methodological problems than studies concerned with only one portion of the life span. Experimental control in this area is more difficult to realize, because the animals, by the very nature of the problem, are exposed to numerous variables, some of which may not yet be recognized or capable of isolation. These difficulties, however, are more than offset by the general interest and wide significance of the problems that may be studied.

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SEQUENTIAL ANALYSIS IN PSYCHOLOGICAL RESEARCH

DONALD W. FISKE AND LYLE V. JONES[1]
The University of Chicago

The purpose of this paper is to introduce sequential analysis to psychological researchers, and more incidentally, to discuss the operating characteristic function for statistical tests in general. Sequential analysis is not presented adequately in the statistical texts most commonly consulted by psychologists and has rarely been used in psychological research. Yet it is a statistical method that has the important advantage that it minimizes the average number of observations required to reach a specific statistical decision; probably it will be most useful to research on personality and other topics where the cost per unit (i.e., per observation or pair of observations) is high. The approach to statistical problems in terms of the operating characteristic or power of a test also has received little attention in textbooks on psychological statistics (cf. **8**, pp. 65–67). This approach, which is important in sequential analysis, has general relevance to statistical tests of hypotheses.

Sequential analysis is fully presented in a book written by its principal inventor, Wald (**14**). A brief technical presentation can be found in Mood (**11**). A more definitive and detailed exposition, presented in the context of sampling inspection, is also available (**3**). The chapter discussing it in Dixon and Massey (**5**, ch. **18**), while presenting sequential procedures in somewhat different form, may be the most readily available introduction for psychological investigators.

The few relevant papers using sequential analysis have essentially been demonstrations of the method as applied to test construction and test administration. Cowden (**4**) gave a final course examination sequentially, with each student being required to answer successive sets of questions until a decision could be reached concerning his achievement. Moonan (**12**) suggests possible uses of the method for problems involving the optimal number of items or for spotting extreme students, but concludes that the method is usually not worth while in education, where the expense of collecting data is relatively independent of the number of items used. The method was applied by Kimball (**10**) to the problem of checking and rescoring test papers. The application of sequential sampling to item selection is shown by Walker (**15**), who, with Cohen (**16**), provides facilitating tables. Schmid (**13**) offers another approach to the same problem. Good agreement is reported by Anastasi (**1**) between decisions about item selection based on sequential analysis and those based on correlations for a total sample.

THE OPERATING CHARACTERISTIC FUNCTION

The typical elementary text in psychological statistics defines the probability of errors of the first kind, α, and the probability of errors of the second kind, β. Errors of the first kind (Type I errors) are errors of rejecting, on the basis of sample observations, a hypothesis, H_0, when

[1] The authors are indebted to Professor W. Allen Wallis and Dr. Esther Seiden for their valuable criticisms of an early draft of this paper.

H_0 is, in the population, true; errors of the second kind (Type II errors) are errors of accepting H_0, when in the population an alternative hypothesis, H_1, is true. Having presented such definitions, it is the rare text in psychological statistics which further discusses the importance of errors of the second kind. Rather, discussions of actual statistical tests of hypotheses emphasize confidence levels, α errors, to the exclusion of the probability of errors of the second kind, β errors.

The operating characteristic (OC) curve, for any statistical test of hypotheses, shows the relationship between the probability of accepting a particular hypothesis and the true value of the parameter under consideration. The height of the OC curve at a given parametric value, μ, represents the probability of accepting the specified hypothesis when the observed sample of observations has been randomly selected from a population with the parameter μ. The use of the OC curve is exemplified in Fig. 1.

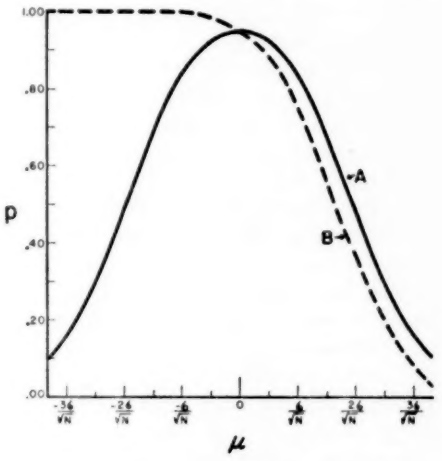

FIG. 1. ILLUSTRATIVE OC CURVES FOR ONE-TAILED AND TWO-TAILED TESTS

Let us suppose that we are drawing a large random sample from a normally distributed population. Let us choose a level of significance, $\alpha = .05$, for our test. In Fig. 1 are exemplified two OC curves, one for each of the following two tests:

1. We wish to test the hypothesis that the population mean is zero ($H_0: \mu = 0$) against the alternative hypothesis that μ differs from zero. The height of curve A, in Fig. 1, gives us the probability of accepting the hypothesis H_0 for all possible parametric values of μ. For example, if the true population mean is zero, the probability that we will accept this hypothesis is .95, or, in general, $1-\alpha$. If the true population mean differs from zero by three standard errors of the mean, the probability that we would accept H_0 is reduced to approximately .16. This value is equal to β, the error of the second kind, for the particular alternative hypothesis $\mu = 3\sigma/\sqrt{N}$. In general, the height of the OC curve is β for any point along the abscissa other than that point included in the specified hypothesis, H_0.

2. We wish to test the hypothesis $\mu = 0$ against the alternative hypothesis $\mu > 0$. The probability of accepting this hypothesis, as a function of the true parametric mean, is given by the height of OC curve B in Fig. 1. Again that height at $\mu = 0$ is .95 or $1-\alpha$. At $\mu = 3\sigma/\sqrt{N}$ that height is .08. For the particular alternative hypothesis $\mu = 3\sigma/\sqrt{N}$, the β error for this test is half the magnitude of the β error for test 1 above. For this alternative hypothesis, this test is more "powerful" than test 1. In general, the "power" of a statistical test is defined as $1-\beta$ for any particular alternative hypothesis; while from an OC curve, one reads the magnitude of β; from a "power curve," one reads the magnitude of $1-\beta$.

If, in this hypothetical problem, we were primarily interested in guarding against the alternative hypothesis $\mu = 3\sigma/\sqrt{N}$, then we would choose test 2 rather than test 1, thus reducing the probability of an error of the second kind. Such a choice would preserve the confidence level, $\alpha = .05$, but would maximize the power of our test. Often, in designing statistical tests of hypotheses, we have a choice among a number of alternative tests. Our choice can be optimally resolved if we know the OC function for each of the tests, for then we can choose that test which maximizes the power (minimizes β) for a particular alternative in which we may be especially interested.

An Example of the Sequential Method[2]

The general approach used in sequential analysis may now be illustrated with a hypothetical example. Suppose that we wished to evaluate a new psychotherapeutic technique. Let us assume that an acceptable criterion for therapeutic success is available and that it has been established that the best one of the conventional techniques is successful in 60 per cent of cases treated. We are interested in determining whether or not the new technique is more successful than the conventional one.

To make a test on these bases, we must be able to observe results of the new technique which may be considered random representations of a population of results from this technique, and which can be reliably scored as successes or nonsuccesses. Let us assume that these requirements have been met. Our sequential plan will consist of examining in sequence single results or groups of results, then of making one of three possible decisions: (*a*) that the new technique is no better than the conventional one, (*b*) that the new technique is better than the conventional one, or (*c*) that we do not have sufficient evidence to make either decision without too great a risk of error. If the third decision is taken, another result or group of results is observed, and on the basis of this additional observation, the three decisions are reconsidered. Sampling proceeds in this manner until we have a basis for making either decision *a* or decision *b*.

In general, the plan for sequential sampling can be expressed in pictorial form, as in Fig. 2. For this binomial problem, the cumulative proportion of successes is plotted against the total number of observations. So long as the plotted line remains in the region bounded by the two parallel straight lines, sampling continues. Should the plotted line reach the upper straight line, the hypothesis under test is rejected in favor of the

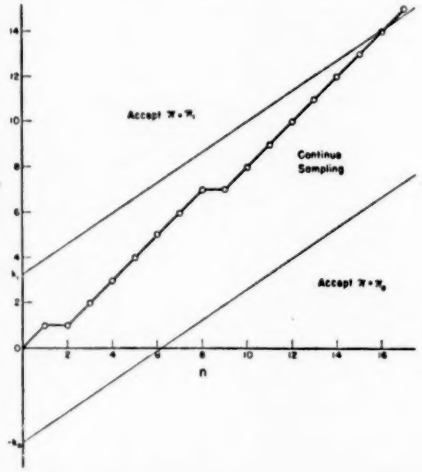

Fig. 2. A Sequential Sampling Plan for Testing Hypothesis about Proportion against One-sided Alternative

[2] The succeeding presentation closely parallels that in (3).

alternative. Should the plotted line reach the lower straight line, the hypothesis under test is accepted. The slope and the intercepts of the two parallel lines are completely predetermined by characteristics of the statistical test.

Now let us return to our example. From the formulation of the problem, one of the hypotheses in which we are interested is obvious, i.e., that the new technique produces no more successes than the old one. In formal terms, we wish to test the hypothesis

$$H_0\colon \pi = \pi_0 = .60,$$

where π is the proportion of successes in the population from which our sample is drawn. The choice of an alternative hypothesis depends upon what we mean by "better than." In this case, we might decide that if the new technique is better than the old, as evidenced by a 15 per cent increase in successes, such a difference would be of considerable importance; we might also decide that a less dramatic difference than this might be of questionable practical importance. On the basis of such considerations, we would choose a second hypothesis

$$H_1\colon \pi = \pi_1 = .75.$$

We now choose a risk α of rejecting H_0 when H_0 is true, and a risk β of accepting H_0 when H_1 is true. In our example, we might choose to make β the smaller risk of the two, for it would be a costly mistake to conclude that the new technique was no better than the old, when actually the new technique produced 75 per cent successes as against 60 per cent for the old. We might then choose

$$\alpha = .10,$$

and

$$\beta = .05.$$

Having chosen the four values H_0, H_1, α, and β, we have completely determined the characteristics of our sequential test and are able to consider the OC curve for the test. To draw a rough sketch of a sequential OC curve, we are able to make use of the following five points, where L is the height of the OC curve:

$a.$ $\quad L(\pi = 0) = 1;$

$b.$ $\quad L(\pi = \pi_0) = 1 - \alpha;$

$c.$ $\quad L(\pi = s) = \dfrac{k_1}{k_0 + k_1};$

$d.$ $\quad L(\pi = \pi_1) = \beta;$

$e.$ $\quad L(\pi = 1) = 0;$

where,

$$s = \frac{\log\left(\dfrac{1 - \pi_0}{1 - \pi_1}\right)}{\log\left[\dfrac{\pi_1}{\pi_0}\left(\dfrac{1 - \pi_0}{1 - \pi_1}\right)\right]}, \qquad [1]$$

$$k_0 = \frac{\log\dfrac{1 - \alpha}{\beta}}{\log\left[\dfrac{\pi_1}{\pi_0}\left(\dfrac{1 - \pi_0}{1 - \pi_1}\right)\right]}, \text{ and} \qquad [2]$$

$$k_1 = \frac{\log\dfrac{1 - \beta}{\alpha}}{\log\left[\dfrac{\pi_1}{\pi_0}\left(\dfrac{1 - \pi_0}{1 - \pi_1}\right)\right]}. \qquad [3]$$

A number of additional points on the OC curve can be found; for a thorough explanation of methods of determining the OC curve, the reader is referred to the report of the Statistical Research Group, Columbia University (3).

In the present example, the OC curve is as sketched in Fig. 3. We are now able to visualize the characteristics of our sequential test, in terms of the OC curve, and to decide whether

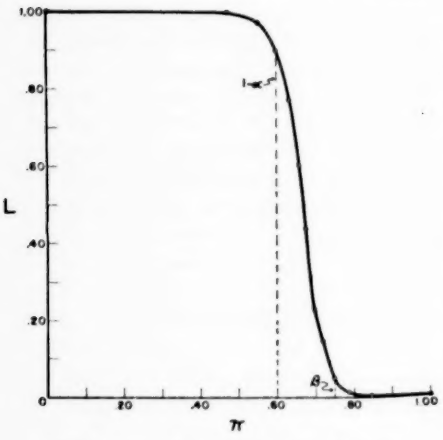

FIG. 3. THE SEQUENTIAL OC CURVE FOR BINOMIAL EXAMPLE

these characteristics are satisfactory for our purposes. If they are not, we alter H_0 or H_1 or α or β in such a manner as to make the test more suitable. If the characteristics are acceptable, we proceed further in determining the sampling plan.

Referring to Fig. 2, the decision regions are completely defined by a a value for the slope of the two parallel lines, and by an intercept value for each of the two lines. In general, the equations of the two parallel lines may be written

$$r_0 = -k_0 + sn \qquad [4a]$$

$$r_1 = k_1 + sn, \qquad [4b]$$

where r stands for the number of successes, n the cumulative number of observations, s the slope, and $-k_0$ and k_1 for the intercepts. It can be shown that s, k_0, and k_1 are the values defined previously in equations [1], [2], and [3] (3, sec. B.072). For the present example, $k_0 = 4.170$, $k_1 = 3.248$, and $s = .678$. Our sequential plan is that represented by the two parallel lines of Fig. 2.

To illustrate an application of sequential analysis to a binomial problem, consider the following sampling experiment. Observations are selected from a table of random numbers. Assuming a population π of .80, we record any digit from 1 to 8 a success (a "1"); digits 9 and 0 are recorded as failures ("0"). A sequence of such observations has been made, and the results are presented in the order observed:

1 0 1 1 1 1 1 1 0 1 1 1 1 1 1 1.

This sequence of observations is graphed in Fig. 2; since the plotted line reaches the upper straight line on the sixteenth observation, sampling stops at that point, and we reject H_0 in favor of H_1. We conclude it to be more likely that $\pi = .75$ than that $\pi = .60$, the confidence in the conclusion being specified by the probability of Type I error, $\alpha = .10$.

THE AVERAGE SAMPLE SIZE

In addition to the OC curve for a sequential test, there is another curve of considerable interest. This is a curve of average sample size (or average sample number, ASN) as a function of the true population parameter. It is useful to refer to the ASN curve to determine the expected cost, in terms of sample size, of achieving that assurance of a correct decision selected from the OC function.

The average sample size depends upon α and β; the smaller these risks, the greater the size that the sample will usually be before a decision is reached. But the ASN also depends upon the hypothesized values π_0 and π_1, and upon the population proportion of successes, π. For given values of α, β, π_0, and π_1 we can write the ASN as a function of π. If π corresponds to the hypothesized proportion, π_0, we have

$$\text{ASN}\ (\pi = \pi_0) = \frac{(1-\alpha)k_0 - \alpha k_1}{s - \pi_0}.$$

If π corresponds to π_1,

$$\text{ASN } (\pi = \pi_1) = \frac{(1 - \beta)k_1 - \beta k_0}{\pi_1 - s},$$

where s, k_0, and k_1 are those values given in equations [1], [2], and [3]. These two values, ASN $(\pi = \pi_0)$ and ASN $(\pi = \pi_1)$, are larger than any average sample size necessary to make a decision if π is not between π_0 and π_1.

Three additional points on the ASN curve may be found to be:

$$\text{ASN } (\pi = 0) = \frac{k_0}{s},$$

$$\text{ASN } (\pi = s) = \frac{k_0 k_1}{s(1 - s)},$$

$$\text{ASN } (\pi = 1) = \frac{k_1}{1 - s}.$$

Thus, just as we were able to sketch the OC curve from five points, given any sequential plan, we are able to sketch the ASN curve from five points. The ASN curve for our earlier example is given in Fig. 4. For any sequential plan it is not difficult to solve for a larger number of points on the ASN curve (**3**).

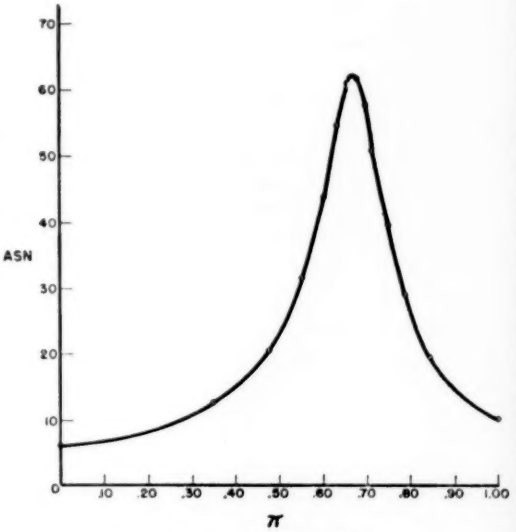

FIG. 4. THE ASN CURVE FOR BINOMIAL EXAMPLE

SOME OTHER SEQUENTIAL TESTS

Requirements regarding untested parameters. The simpler forms of sequential test assume that either the variance or the mean is known. Tests involving means and differences between means assume that the variance is known. Similarly, the convenient test of hypotheses involving the standard deviation assumes that the mean is known; a more laborious method is available when it is unknown. (This test for the standard deviation is presented in [**3**, **5**, **14**].)

Hypothesis involving means. Application of the basic equations for testing hypotheses concerning means assumes that observations are drawn from a normally distributed population. These equations, defining the parallel decision lines, are

$$\frac{u_1 - u_0}{\sigma^2} \Sigma X_i + \frac{u_0^2 - u_1^2}{2\sigma^2} n = \log_e \frac{\beta}{1 - \alpha}, \quad \text{and} \qquad [5a]$$

$$\frac{u_1 - u_0}{\sigma^2} \Sigma X_i + \frac{u_0^2 - u_1^2}{2\sigma^2} n = \log_e \frac{1 - \beta}{\alpha} \qquad [5b]$$

where u_0 and u_1 are the means stated in our two hypotheses, H_0 and H_1, respectively, X_i is a single observation, σ^2 is the variance of X, n is the number of observations, and α and β are the specified probabilities of Type I and Type II errors. Since these formulas involve the variance, the experimenter must either have a good estimate of the variance from other data or must take an initial group of cases (n) of sufficient size to yield a

reasonably reliable estimate (s^2) of the variance. If the estimate is too small (or too large), he will be unwittingly increasing (or decreasing) the values of α and β and thus will make his test less (more) sensitive than he has planned to have it.

By solving formulas [5*a*] and [5*b*] for ΣX_i, we obtain the following equations:

$$\Sigma X_i = \log_e \frac{\beta}{1-\alpha} \frac{(\sigma^2)}{(u_1 - u_0)} + \frac{(u_1 + u_0)n}{2}, \quad \text{and} \qquad [6a]$$

$$\Sigma X_i = \log_e \frac{1-\beta}{\alpha} \frac{(\sigma^2)}{(u_1 - u_0)} + \frac{(u_1 + u_0)n}{2}. \qquad [6b]$$

These are the equations for the parallel lines in a graph representing the sequential plan for testing hypotheses involving means. To carry out the test, we plot on the graph the observed ΣX_i for each value of n. When the observed cumulative sum falls outside one of the lines, the appropriate decision is made.

Difference between means. A convenient sequential procedure is available for testing the significance of the difference between means of two equal-sized samples. In almost any instance where the sequential approach would be preferred to a conventional method based upon fixed N's, the use of equal-sized samples is not a serious restriction.

The procedure involves the use of difference scores between randomly selected pairs, or matched pairs if the groups are matched case for case. The sum of these difference scores is plotted on the figure specified by formulas [6*a*] and [6*b*], with u_0 set at zero and u_1 determined by theoretical or practical considerations. Once again, the procedure assumes that the variance is known. The variance used here is, of course, the variance of the difference between means (for correlated or uncorrelated samples, whichever is appropriate).

Two-sided hypotheses involving means. All the preceding tests were one-sided; for example, the tests involving means specified just two values, u_0 and u_1. Even the test of mean difference required a single specific value for the alternative hypothesis. The procedure outlined below is designed to test whether the mean of a group deviates in *either* direction from some specified value. Thus instead of setting one value for the alternative hypothesis, we set two, which deviate the same amount but in opposite directions from the hypothesized central value, u_0. (For a full account, see **3**.)

Let d be the deviation of each of the two means, specified under the alternative hypothesis, from the mean, u_0, specified in the null hypothesis. Let our critical values be

$$a = \log_e \frac{1-\beta}{\alpha}, \quad \text{and}$$

$$b = \log_e \frac{\beta}{1-\alpha}.$$

The formula required for the sequential test of significance is

$$G = \log_e \cosh \frac{d\,|\,\Sigma(X_i - u_0)\,|}{\sigma^2} - \frac{d^2 n}{2\sigma^2}, \qquad [7]$$

where n is the number of observations (as previously), X_i is any observed score, σ^2 is the true variance of the population from which the observations are drawn, and cosh refers to the hyperbolic cosine. Tables of the hyperbolic cosines are accessible (e.g., **2**, Table XV; **3**, Table 5.2). If G is

greater than a, the null hypothesis is rejected in favor of the alternative hypothesis that the mean of the group differs from u_0 by the amount d. If G is more negative than b, we accept the null hypothesis that the mean of the group does not differ from u_0.

For most values, it is unnecessary to use the tables for the hyperbolic cosine because $\log_e \cosh z$ is asymptotic to $z - \log_e 2$ or $z - .693$. The equivalence is exact to two decimal places if $z \geqq 2.7$. Hence we may modify formula [7] as follows:

$$G' = \frac{d\,|\,\Sigma(X_i - u_0)\,|}{\sigma^2} - \frac{d^2}{2\sigma^2}\,n - .693. \qquad [8]$$

We set formula [8] equal first to a and then to b, and solve for $|\,\Sigma(X_i - u_0)\,|$:

$$|\,\Sigma(X_i - u_0)\,| = \frac{d}{2}\,n + \frac{\sigma^2(a + .693)}{d}, \text{ and} \qquad [9a]$$

$$|\,\Sigma(X_i - u_0)\,| = \frac{d}{2}\,n + \frac{\sigma^2(b + .693)}{d}. \qquad [9b]$$

These equations yield the parallel lines in Fig. 5, which has been set up for the following example: $u_0 = 10$, $\sigma^2 = 100$, $d = 6$, $\alpha = .10$, $\beta = .05$. Data for three sampling experiments are plotted on Fig. 5, the values being obtained by drawing random sequential samples from normally distributed populations with $u = 9$, 16, and 20 respectively.

An important consideration must be emphasized. Equations [9a] and [9b] were based upon equation [8], which is an adequate approximation to the precise equation [7] so long as $z \geqq 2.7$, where

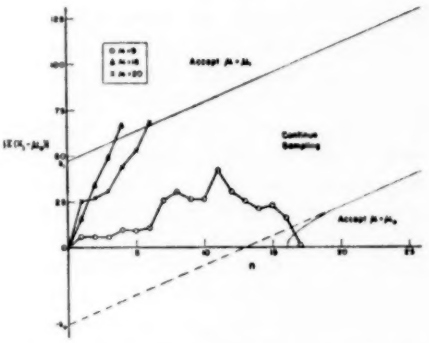

FIG. 5. A SEQUENTIAL PLAN FOR TESTING HYPOTHESIS ABOUT MEAN AGAINST TWO-SIDED ALTERNATIVE

$$z = \frac{d\,|\,\Sigma(X_i - u_0)\,|}{\sigma^2}.$$

For $z < 2.7$, equation [7] must be applied. It is sufficient to evaluate three or four values of G, corresponding to values $0 \leqq z < 2.7$, from equation [7]. These values provide points on the curved portion of the lower decision line of Fig. 5, and allow the necessary correction to become part of the graph of the sequential plan.

In Fig. 5, the sampling experiment for $u = 20$ gives a value of 70.32 at $n = 6$, allowing us to reject $H_0: u = 10$, in favor of $H_1: u = 10 \pm 6$. The sampling experiment for $u = 16$ allows the same decision at $n = 4$. For $u = 9$, it has required an n of 17 to yield a decision, in this case acceptance of H_0 (with probability of erroneous decision, $\beta = .05$).

The application of this method to a two-sided test of the difference between two means is clear-cut. Once again we take pairs of observations, one from each group, and analyze the differences for the n pairs. The value of u_0 is of course zero.

Hypotheses involving correlations. No convenient method is currently available for utilizing sequential

analysis in tests of hypotheses involving correlations. (It is possible that the labor involved in the computation of correlations may offset the advantages of sequential design.) While the product-moment correlation coefficient, r, is a mean, the mean cross product of the pairs of observations in standard score form, the sampling variance is a function of the population correlation and therefore would differ for two hypothesized correlation coefficients.

In many cases, a relationship can be measured by some statistic other than r. However, the nonparametric statistics, rho and tau, are unsatisfactory for sequential tests. For each, as for r, the variance is a function of the relationship in the population. Furthermore, the sampling distributions of rank coefficients for the nonnull case are complex; the approximation in the case of tau (**9**, chs. 4 and 5) seems too crude for use in sequential tests.

In this connection, Wolbers (**17**) has suggested a procedure for estimating correlation coefficients as data accumulate. In the method he suggests, each variate is dichotomized at the median. He provides a table for estimating the relationship from the number of cases in the accumulated sample and the number of cases falling on different sides of the two medians (e.g., above the median on X and below the median on Y). However, this method yields an estimation. It is not a sequential test of hypotheses.

A general test of hypotheses involving double dichotomies. A sequential test is available for determining which of two groups has the higher proportion of some specified condition. This test applies to the general case of testing differences in proportions of "successes" in two samples. It could have been used for an experiment on the problem involved in our original example, that of comparing the proportions of successes produced by two therapeutic techniques. Patients would be assigned randomly to two groups, one for each type of therapy, and the proportions of successes in each would be ascertained empirically.

Such a test, for difference between proportions, should be contrasted with that test applied in the earlier example to test the significance of the deviation of a sample proportion from an a priori value. Since the test model for double dichotomies is more complex than the methods previously discussed, the reader is referred to the original source for details (**3**, sec. 3).

General Remarks

The selection of the alternative hypothesis. The alternative hypothesis tested in a sequential test specifies a parameter other than that given by the null hypothesis. How does the experimenter select this value? We may distinguish two general situations. In the first situation the experimenter can specify a value for H_1 on one of three particular bases. (*a*) He may be testing a theoretical estimation of the parameter. Rarely would this basis be available to the psychologist, since current theory does not seem sufficiently precise. (*b*) He can choose a value on the basis of previous empirical findings. (*c*) He can decide on practical grounds that the parameter must be at or beyond the specified value because lesser values would make no practical difference. Some of the earliest applications of sequential analysis were to problems of sampling inspection where the parameter presumably was chosen on such grounds.

But another general situation may exist where none of the above bases

for decision is available or appropriate. The experimenter may wish to use a sequential test for the sole purpose of economy of effort. What rationale can he follow? For these circumstances, we suggest a plan based upon a rough approximation to the largest number of observations that the experimenter is willing to use. Having chosen a trial H_1 such that for selected values of α and β the OC curve is satisfactory, the experimenter can compare the highest point on the ASN curve with his preferred maximum N. If the maximum ASN exceeds the preferred maximum N, a second value of H_1, more distant from H_0 than the first, may be chosen and new OC and ASN curves drawn.

The chance that the actual sample size required to make a decision will exceed the maximum ASN is small, definitely below .5. Usually one of the hypotheses will be accepted long before the sample size reaches the maximum ASN, and often when the number of observations is less than half that size.

It is obvious that the closer together the values postulated in H_0 and H_1, the larger the ASN (cf. Fig. 4). As in conventional statistics, small effects are more difficult to isolate than large ones. On the other hand, if the difference between the two hypothesized values is unreasonably large, the experimenter gains little by deciding to accept one hypothesis in preference to the other.

Even when an alternative hypothesis can be determined on a priori or practical grounds, it is advisable to consider the OC and ASN curves in planning a sequential test. Only in this way can an experimenter be completely assured that his design is one which actually will provide the test he desires at an acceptable cost (moderate ASN).

Sequential tests vs. conventional tests. Does a sequential test have any advantages when the selection of a second hypothesis is done on an essentially arbitrary basis? The chief advantage is economy. Sequential analysis produces an *average* saving in sample size of about one-third to one-half, when compared with the most powerful current statistical tests. However, the experimenter using sequential analysis will use *more* cases about 25 per cent of the times he uses this test if $\alpha=\beta=.05$. (Rarely will he have to use twice as many cases.) If $\alpha=\beta=.01$, the probability is about .9 that he will effect a saving (cf. **14**, p. 57 and p. 60).

This advantage can be seen intuitively in our first example. Here, the two hypotheses stipulate the proportion of successes at .60 and .75 respectively. If our first eight randomly selected cases had included no failure case, it would be very unlikely that the observed sample p subsequently would drop to a value so low as to lead to acceptance of $H_0: \pi = .60$. On the other hand, if six of the eight were failures, it would be unlikely for subsequent sampling to produce a p so high as to lead to acceptance of $H_1: \pi = .75$. Such extreme results are uncommon. However, sets of observations showing some tendency toward an extreme occur sufficiently often to make the method economical. Sequential analysis takes advantage of the fact that some possible sets of n observations make improbable (or impossible) certain obtained values for $n+1$, $n+3$, or even $3n$ observations.

An evaluation of the sequential design. The uncritical general use of sequential analysis obviously is not recommended. It is a design which can have advantages when one or more of the following conditions

holds: (*a*) The problem involves the choice between two possible parameter values which can be specified on a priori but not arbitrary grounds (see "The Selection of the Alternative Hypothesis," above). The null hypothesis will usually be one of the two. (*b*) The data are such that the cost per datum is high and economy is desired. (*c*) The total amount of data is not fixed. In this situation, the experimenter takes a calculated risk. We have seen above that in a small proportion of applications of sequential tests, he will require more cases than he would if he used a more conventional statistic. However, the odds are great that he will economize on his data collection.

As Wallis (**7**, ch. 17) points out, in the nonsequential test, N is fixed, and with one risk specified (α), the second risk (β) is minimized. In the sequential test, we specify both risks (α and β) and minimize the sample size.

In a situation where we are able to specify both α and β, but where it is uneconomical to collect observations piecemeal, it may be preferable to estimate the desirable sample size and proceed with a conventional study, collecting and analyzing all data at the same time (cf. **5**, ch. **14**, especially pp. 212–213 and pp. 219–220; also **6**, pp. 153–157). For this latter situation, the total effort required to reach a decision can be estimated in advance. In a sequential test, such an estimate will necessarily be only a rough approximation.

Summary

This paper seeks to introduce sequential analysis to psychological researchers as a statistical design that has considerable promise for work in areas where the cost per observation is high.

The basic design involves the testing of alternative hypotheses, one of which is usually a null hypothesis. The experimenter sets up three alternative decisions, one of which is accepted after each set of data is analyzed: (*a*) he may accept the first hypothesis; (*b*) he may accept the second hypothesis; (*c*) he may accept neither and take another observation. If he makes the third decision, he makes another observation (or set of observations) and then repeats the analysis. This procedure is followed until he accepts one hypothesis or the other.

In its more convenient forms, the sequential test assumes knowledge of the population variance or (in tests of variance) knowledge of the population mean. The applications of the statistical design to problems involving proportions, means, and mean differences are illustrated. Methods are given for sketching operating characteristic and average sample number curves for sequential tests. The use of these curves is exemplified and their value is emphasized.

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SPEED OF LEARNING AND AMOUNT RETAINED: A CONSIDERATION OF METHODOLOGY

BENTON J. UNDERWOOD[1]
Northwestern University[2]

A common generalization found in textbooks is that the fast learner retains more than the slow learner. McGeoch (**6**), apparently on the basis of Gillette's work (**1**) and on the basis of his general conception of learning and forgetting processes, wrote: "This high positive relation between individual scores in learning and retention is to be expected, of course, from the fact that these are continuous processes" (**6**, p. 388). Although not explicitly stated in terms of rate of learning and forgetting of individuals, the present writer has also assumed the validity of the general principle that speed of learning and amount retained are directly related (**8**, p. 510). Kingsley (**4**, p. 469), Munn (**7**, p. 213), and Hilgard (**2**, p. 10) have also accepted the generalization.

The present paper has two purposes. First, we will show that the methods used to obtain the data from which this principle was derived are inappropriate. Secondly, we will show that when suitable methodology is applied to the problem, there is no difference in rate of forgetting of fast and slow learners.

METHODS USED BY PREVIOUS INVESTIGATORS

The last extensive work on the problem of the relation between speed of learning and amount retained was Gillette's monograph (**1**) published in 1936. It is highly probable that this monograph is responsible for the general conclusions appearing in textbooks as noted above. In this monograph Gillette critically summarizes the methods and results of all previous investigations, going back as far as Ebbinghaus. Two methods of investigation had been used in these previous investigations. Gillette's criticisms of these methods may be noted briefly.

Method of equal amount learned. By this method all *S*s learn a task to a given criterion of proficiency, and after a rest interval retention measurements are obtained. The most commonly used statistic is the correlation coefficient between a measure of time to learn to the standard criterion and the recall or relearning scores. Gillette points out that this technique favors retention of the slow learner since certain of the items or parts of the task will be much overlearned and therefore might be expected to enhance retention. This would tend to reduce differences in retention between fast and slow learners. Indeed, correlations near zero between time to learn and amount recalled are interpreted by Gillette as actually indicating the slow learners have forgotten more rapidly than fast learners. Nevertheless, Gillette rightfully rejects this method as a means of reaching a definitive conclusion on the issue.

Method of equal opportunity to learn. By this method all *S*s are allowed the same amount of time to study the

[1] Much appreciation is due Jack Richardson, my research assistant. He has helped with the analyses and is entirely responsible for fitting the curves of Fig. 1. He has also critically read the manuscript.

[2] The work was done under Contract N7onr-45008, Project NR 154-057, between Northwestern University and the Office of Naval Research.

material before the retention interval. Again, Gillette notes that this method is quite inappropriate because the fast learners will learn more than the slow learners and therefore may be expected to have higher retention simply because they have learned more. In general, the studies she reviews show fairly high correlations between amount learned and amount retained, and her own data resulting from this technique likewise yield high correlations. Actually, of course, using this technique, fast *S*s could show more rapid forgetting than slow *S*s, and the high correlations would still obtain if there were wide differences in amount originally learned. This method, therefore, must be rejected as a means of investigating the problem.

Method of adjusted learning. Gillette's solution to the methodological problem was to use the adjusted-learning technique. By this technique items in a list are "dropped out" after being learned to a minimum criterion (such as one correct anticipation). Thus, all items of a list are given the same number of "reinforcements." And, while slow learners would take more time to obtain these reinforcements than would fast learners, Gillette assumed that the strength of associations at the end of learning was the same for fast and for slow *S*s.

Gillette's conclusion from her experiment using the method of adjusted learning is as follows: "The slow learner when given sufficient time to learn the *same amount* as the fast learner, but not allowed to *overlearn* the material, is not able to retain as much as the fast learner" (**1**, p. 50). Her final conclusion, after using all three methods, was that there are very clear indications that the fast learner retains better than the slow learner.

Inadequacy of the Method of Adjusted Learning

The inadequacy of the method used by Gillette can best be understood if a brief résumé is given of certain calculations made on data which were available to us. As a result of a series of investigations on rote learning which we have published elsewhere in connection with other problems (**9, 10, 11, 12**), we had a large amount of data which could be analyzed in an attempt to obtain more information on the relationship between speed of learning and amount retained. These experiments involved both paired-associate and serial learning, and both nonsense syllables and adjectives. The lists were always learned to a criterion of one perfect recitation with recall and relearning occurring after 24 hr.

Using these data, we first obtained correlations between trials to learn and recall, and between trials to learn and relearn. Since our *S*s all learned to the same criterion, the method is the one referred to above as the method of equal amount learned. In general conformance with previous findings summarized by Gillette, correlations between time to learn and amount recalled were all near zero. Correlations between time to learn and time to relearn were high and positive. Viewed by this method, then, our results are quite in accord with previous findings.

Our next step was to analyze the data in such a fashion that we would in effect be using the adjusted-learning technique. We divided a group of *S*s for a given experiment into two equal subgroups based on number of trials to learn, thus giving a group of slow learners and a group of fast learners. We then counted the number of correct anticipations made by each *S* for each item in learning a list

to one perfect trial. Each correct anticipation we call a reinforcement simply because the greater the number of correct anticipations the more resistant is an item to forgetting. All items having the same number of reinforcements were pooled, using separate pools for slow and fast *S*s. Thus, we might have 200 items which had been given five reinforcements while being learned by slow *S*s and 225 items which had been given five reinforcements while being learned by fast *S*s. The final step was to determine the percentage correct at recall for each frequency of reinforcement for fast and slow *S*s. This is, in effect, comparable to Gillette's adjusted-learning technique.

Results of such comparisons between fast and slow *S*s for several experiments showed consistently that for items equated for number of reinforcements during learning, retention was consistently superior for fast learners. This was true regardless of the actual number of reinforcements obtained during original learning. That is, fast *S*s were superior in recall for items having only a few reinforcements and for items having many reinforcements. Thus, this analysis gave strong empirical confirmation to Gillette's findings.

The assumption on which acceptance of Gillette's method (and of our modification of it) rests is that the making of a correct anticipation (a reinforcement) by a slow *S* results in the same associative strength as a reinforcement for a fast *S*. If this assumption is not valid, comparisons of retention of fast and slow learners are as misleading by this method as by the other methods thus far discussed. For, if a reinforcement adds more strength to an association being acquired by a fast *S* than it does to one for a slow *S*, it means that degree of learning of the material is not equal before introduction of the retention interval. Comparisons of retention, therefore, are distorted in the same fashion as with the other two methods discussed above.

In order to make comparisons of retention of fast and slow learners, it is absolutely essential that the response strengths of the items for fast and slow learners be equated at the end of learning—at the start of the retention interval. The method of adjusted learning and our variation on this method do not assure that this is the case. Indeed, it is a very reasonable hypothesis that the essential difference between fast and slow learners is that a reinforcement *does* result in more associative strength for a fast *S* than for a slow *S*. Thus, the better retention of fast *S*s as found by Gillette and also by our method may be entirely a function of the unequal degree of learning before the retention interval was introduced.

Technique for Equating Degree of Learning for Fast and Slow Subjects

The solution to the problem results from what we call a successive probability analysis of learning. The data we will use to demonstrate the method come from five experiments on the learning and retention of paired nonsense syllable lists (**10**). Each experiment employed 36 *S*s. We have divided these into two groups of 18 each based on number of trials to learn. Each *S* learned and recalled three lists, the conditions for learning differing only in terms of intertrial interval. These intervals (4, 30, and 60 sec.) produced no difference in learning, and they do not interact with *S*'s ability level. Therefore, we may treat them as three equivalent learning tasks for each *S*.

Since there were five experiments, and since we get 18 fast *Ss* and 18 slow *Ss* from each experiment, we have a total of 90 fast and 90 slow *Ss*, each having learned and recalled three lists. Effectively, then, we have 270 lists for each group, and since each list had 10 pairs, we have 2,700 items for each group.

The successive probability analysis consists of determining the growth of the associative function for each item. We first observed when an item was correctly anticipated initially. We then noted whether the response was anticipated correctly or incorrectly on the immediately following trial. When an item had been anticipated correctly twice (whether on successive trials or not), we noted whether it was correct or incorrect on the next trial. This continued until the item was analyzed for the entire course of learning. As an illustration, suppose that an item was first given correctly on trial 6. On trial 7 it was also given correctly, on trial 8 incorrectly, trial 9 correctly, trial 10 correctly, with *S* also getting all other items correct on trial 10. Our entries would be: after 1 reinforcement, it was correct on next trial; after 2 reinforcements it was incorrect on next trial; and after three reinforcements, correct.

When such an analysis is made for each item for a large number of *Ss*, and when these data are pooled, we are in a position to make exact statements of the probability of getting an item correct on a succeeding trial when this item has been correctly anticipated on previous trials once, twice, three times, etc.

As can be seen, the labor involved in making this successive probability analysis is great. To reduce the labor somewhat we eliminated some items which were very easy to learn and hence had been given a great many reinforcements. Working first with data for slow *Ss*, we eliminated all items which had been correctly anticipated more than 31 times by *S* in learning a list. Out of the 2,700 items, 289 were eliminated by this criterion. We then made a distribution of number of reinforcements given the 2,700 items by the fast *Ss*. On the assumption that easy items for slow *Ss* would also be the easy items for fast *Ss*, we eliminated approximately 289 of the easiest items for the fast *Ss*. The actual number was 322 since we did not want to eliminate a portion of a category, as would have been necessary if we had eliminated exactly 289 items. For these fast *Ss*, the elimination of 322 items left only items which had been reinforced less than 18 times.

The successive probability curves for fast and slow *Ss* are plotted in Fig. 1. The number of observations decrease as the number of reinforcements increase. For the fast *Ss* a total of 18,379 observations are involved, with 2,372 for the case of one reinforcement and 54 for the 17 reinforce-

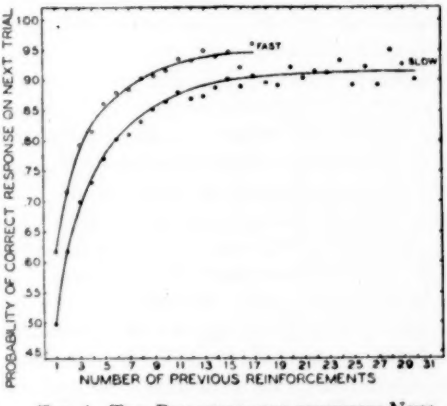

FIG. 1. THE RELATIONSHIP BETWEEN NUMBER OF REINFORCEMENTS DURING LEARNING AND THE PROBABILITY OF CORRECT RESPONSE ON NEXT TRIAL FOR FAST AND SLOW LEARNERS

ments. For the slow *S*s 31,560 observations were totalled, with 2,403 for one reinforcement and 28 for 30 reinforcements. The amount of labor involved in compiling the data for the curves has some compensation in the smoothness of the functions.

From Fig. 1 it is clear that a reinforcement on a given trial (or series of trials) does not result in equal probabilities that that response is correct on the next trial for fast and slow *S*s. Following one reinforcement, for example, the probability is .62 for a fast *S* that the item will be correctly anticipated on the next trial. For the slow *S*s the value is .50. Such differences occur throughout the entire range of reinforcements plotted. It is thus apparent that studies which have "equated" fast and slow learners by equating number of reinforcements have not equated for associative strength. Therefore, our next step is to equate associative strength at the end of learning so that comparable retention measurements may be made for fast and slow *S*s.

It can be seen from Fig. 1 that the equation of associative strength could be made graphically. Thus, we see that six reinforcements for slow *S*s result in about the same associative strength during learning as do three reinforcements for fast *S*s. Other comparable values could be determined and two base lines plotted, one for fast *S*s and one for slow *S*s, in such a fashion that associative strength at the end of learning is equal for the two groups at any point along the base lines. Such adjustments would allow us to say that if these *S*s had been measured on an immediately succeeding trial following the end of learning, the associative strength for fast and slow *S*s would have been the same. Knowing this, we can then make observations of recall with confidence that differences in retention are a function of differences in rates of forgetting of fast and slow *S*s, and not a function of differences in response strength at the end of original learning.

In general, the above procedure has been followed. The critical step, of course, is to equate response strengths along the base line for fast and slow *S*s. To do this we first gave mathematical expression to the curves of Fig. 1. After trying several types of curves, it was found that the exponential-type function gave the best fits. Essentially the formula is that of Hull (**3**, p. 119), with the exception that we have not forced the curves to pass through the origin. The two formulas are:

$$\text{Fast } Ss\text{: } H = 95.6 - 35.20e^{-.1031N}$$

$$\text{Slow } Ss\text{: } H = 92.5 - 40.91e^{-.0801N}.$$

In these expressions H is associative strength. The first values on the right-hand side (95.6 and 92.5) are asymptotes estimated from Fig. 1. The notation e is a constant given the value of 10, and N is the number of reinforcements. These formulas fit the data well except for the first point on each curve. Details of the procedure used in deriving and working with these curves may be found in Lewis (**5**, pp. 61–62).

To construct exactly comparable base lines using the above formulas we used the curve for fast *S*s as a reference curve. The H values for each successive N for fast *S*s were substituted into the formula for slow *S*s and the equation solved for N. This means that we could determine the N value for slow *S*s which gave the same H value for a given N value for fast *S*s. Thus, for each H value we have two N values, one for each group. The two N values for successive H values

are plotted on coordinated base lines in Fig. 2. At any point along these base lines we know that the probability that a response will be correct on an immediately succeeding trial is the same for both groups. In Fig. 2 the highest N value for fast Ss is 10, this being almost equivalent in associative strength to N of 30 for slow Ss.

The final step is to plot the proportion of items recalled after 24 hr. for each group for each level of associative strength. When this is done (Fig. 2) we see that no consistent difference exists between the two curves. We conclude that when associative strength at the end of learning is equivalent for slow and fast Ss, no difference in recall after 24 hr. may be expected. This holds true for a wide range of associative strengths. Because forgetting is rather great in both groups, and because it does not vary between groups as a function of associative strength, we would not expect differences to emerge with longer retention intervals.

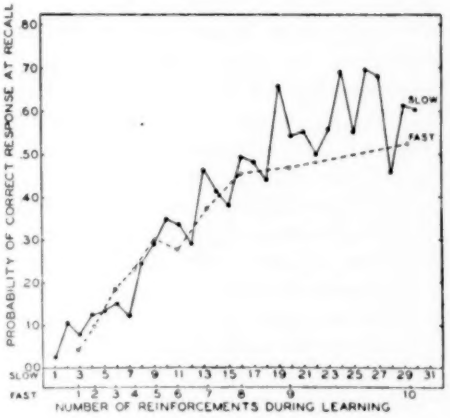

FIG. 2. RETENTION OF FAST AND SLOW LEARNERS AFTER 24 HR. WHEN ASSOCIATIONS ARE EQUATED FOR STRENGTH AT THE END OF LEARNING

OTHER IMPLICATIONS

The results of the above analyses lead to a strong temptation to assert that the critical difference between fast and slow Ss is that the associative strength resulting from a reinforcement is less for slow than for fast learners. In a descriptive sense this is true, as indicated by Fig. 1. However, in the theoretical sense some reservation is necessary. Between successive presentations of a given item during learning, a maximum of about 1.5 min. elapsed. This interval might be as low as a few seconds depending upon the location of the item in successive presentations of the list and upon the intertrial interval used. It might be argued that if there is no difference in forgetting between fast and slow Ss over 24 hr., it is highly unlikely that there are differences in forgetting over these very short intervals between successive presentations of the same item during learning. Nevertheless, it is possible that a reinforcement for a fast and a slow S adds the same associative strength, but that in the context of learning a group of items (as contrasted with the relative rest over 24 hr.) more forgetting might take place for an item for slow Ss than for fast Ss over these short intervals between trials. Thus, it must not inevitably follow that reinforcements add different associative strengths at the time they occur for fast and slow Ss; we believe they do add or produce different amounts of associative strength but our data are not conclusive on this matter. We can conclusively say only that when probabilities of response for successive trials during learning are equated for fast and slow Ss, no difference in forgetting occurred.

The relationships between other variables and forgetting may, if ana-

lyzed by the present technique, be different from those commonly believed to be true. However, such analyses may be expected to be fruit ful only if differences produced by the variable during *learning* are fairly large. For example, it is generally believed that the more meaningful the material the less rapid the forgetting. Meaningfulness *does* produce large differences in rate of learning. However, it is quite possible that if materials or different degrees of meaningfulness were equated for associative strength at the end of learning, no differences in rate of forgetting would be demonstrable. It goes without saying that if such findings do occur we will need to shift our ideas when constructing theories of forgetting.

Summary

A review of previous methods used to study the relationship between rate of learning and rate of forgetting showed that none was adequate to the problem. In no case was it established that associative strength before the retention interval was equal for *Ss* learning at different rates.

A method for equating associative strength at the termination of learning was described and applied to data obtained in learning and recalling paired nonsense syllables. With such equality established it was shown that no difference in forgetting occurred over 24 hr.; the recall of slow *Ss* was as good as that of fast *Ss*.

The method may be useful for studying the influence of other variables on retention.

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SPECIAL REVIEW

SOME RECENT BOOKS ON PERSONALITY

DAN L. ADLER
San Francisco State College

BLUM, GERALD S. *Psychoanalytic theories of personality.* New York: McGraw-Hill, 1953. Pp. xviii+219. $3.75.

EYSENCK, H. J. *The scientific study of personality.* New York: Macmillan, 1952. Pp. xiii+320. $4.50.

EYSENCK, H. J. *The structure of human personality.* New York: Wiley, 1953. Pp. xix+348. $5.75.

HARROWER, MOLLY. *Appraising personality.* New York: Norton, 1952. Pp. xvii+197. $4.00.

JERSILD, ARTHUR T. *In search of self.* New York: Columbia Univer. Teachers Coll., 1952, Pp. xii+147. $2.75.

NOTCUTT, BERNARD. *The psychology of personality.* New York: Philosophical Library, 1953. Pp. 259. $4.75.

NUTTIN, JOSEPH. *Psychoanalysis and personality.* New York: Sheed & Ward, 1953. Pp. xiv+310. $4.00.

PATTY, WILLIAM L., & JOHNSON, LOUISE SNYDER. *Personality and adjustment.* New York: McGraw-Hill, 1953. Pp. viii+403. $4.75.

SPINLEY, B. M. *The deprived and the privileged.* London: Routledge & Kegan Paul, 1953. Pp. vii+208. $4.00.

VERNON, PHILIP E. *Personality tests and assessments.* London: Methuen, 1953. Pp. xi+220. 18s.

Introduction: A Frame of Reference

If, as Smith (**4**) maintains, social psychology attained its rapid growth rate and transformation during recent postwar years, then it can be said that the study of personality lost its momentum during approximately this same period. Indeed, the latter seems now to have come to another node of its epicycloidal career, although not quite back to its starting point. A look at this last cycle will give some perspective to the reviews that follow.

The decade preceding the war was characterized by an exhilarating drive to explore the whys and wherefores of behavior. The air was alive with dynamic theories, "models," and experimental thrusts into the nature of motivated action. Genotypical soul mates like Tolman's "sowbug" and Lewin's "baked potato" vied with Hullian *habs* under the watchful eyes of mathematicians, physiologists, and logical positivists. What quarreling existed was intellectually sound and was conducted, in the main, under Marquis of Queensbury rules of scientific inquiry. This was a period when evolutions inherent in the development of psychology reached apogee. It was then, for example, that atomism seemed finally to be unseated by molar interdependence, when instruments became less important than behaviors, and when classification and empirical law were supplanted by theory. It was then that psychology embraced the hypothetico-deductive method of science and began truly to shed its metaphysical trappings.

It is difficult to recognize these prewar antecedents in postwar psychology, particularly in the area of personality. There is, of course, no less concern with the causes and courses

of behavior, but the method of explication has undergone radical change. A major divergence has become explicit. There is, on the one hand, an experimental group involved in the mechanics of proof, operating under improved ground rules of statistical design and rigor. On the other hand, there has emerged an equally vigorous clinically oriented discipline that, for the most part, operates with different assumptions and for whom the nature of proof basically seems to be "self-evidence."

For rather different reasons, both groups appear to be contributing to the delinquency of psychology by disinheriting the scientific bent so laboriously acquired during prewar years. The experimentalists' efforts are now all too often vitiated by absorption in details and specious variables. Unlike their predecessors, they seem to have narrowed rather than broadened the breadth of their conceptualization and pursuits. There is a danger that the hypothetico-deductive method will be lost to them for lack of a theoretical vehicle to carry it. The clinical group presents the problem differently. They have not forsaken scientific method—principally because they have never adopted it. Although their explanatory systems have rarely lacked breadth, they have continuously lacked evidence. Where the experimentalist is inclined to recast his system on the basis of a single contraindication, the clinician is apt to "prove" his system by a single bit of affirming evidence.

This picture of contrast between the pre- and postwar psychology of personality is painted, of course, without qualifying "shades of gray." It is not so much a lament for personality psychology as it is a notation of the *status quo* from which we will ultimately move. Thus, the current literature contains not only manifest evidence of the present position as postulated, but hints as well at some lines of future development.

Of the ten books included in this review, six come from other than American psychologists. The four American contributions may be classified basically as compendia of what we know, or think we know, about personality. One is a text (Patty and Johnson), one is a compilation of theories (Blum), and two are "practical guides" for physicians and teachers respectively (Harrower, Jersild).

The European contributors afford some relief to this uniformity, although three of these books are also compilations (Eysenck [1953], Vernon, Notcutt). The remaining three are of a different stripe altogether. One is Eysenck's (1952) recapitulation and development of a factorial theory of personality made known to Americans in 1947 (**2**). Nuttin's book is a declared attempt to reinterpret Freudian doctrine in what the author calls a "nonpathological" sense. Spinley's volume is a research report which differentiates the perceptions and personalities of individuals belonging to two English subcultures.

Eysenck—*Scientific Study of Personality*

Of all the books considered, this is at once the most provocative and the most provoking. It combines the virtue of sophisticated deduction and the vice of blatant bias. Although occasionally it carps and quibbles, the unfolding of its theme tantalizes to the very end.

Eysenck sets the stage in the first chapter with a review of the essentials of scientific method, specifically emphasizing antinominalism, conceptualization, and testability. Then,

considering the developmental stages of science, he consigns psychology to a bottom rung of this developmental ladder. Our need, according to Eysenck, is to establish a new and valid taxonomy of personality. The only method that will enable us to do do so now is factor analysis (p. 42). But not any factorial method suffices. Eschewing, for the noncognitive field, the methods of both Thurstone and Cattell, Eysenck presents "Criterion Analysis" as an alternative procedure, which purportedly combines the effectiveness of factor analysis and the hypothetico-deductive method.

Criterion analysis begins with the selection of criterion groups (e.g., normal and neurotic groups) and the administration of what might prove to be discriminating tests. Tetrachoric or biserial correlations with the criterion groups are obtained (criterion correlations) and test intercorrelations determined. Factor coefficients (saturations) are derived from the latter, and their proportionality to the criterion coefficients is examined. The presence of this proportionality is taken as evidence that the criterion factor (e.g., neuroticism) has been isolated *and* identified.

In Chapter 4, Eysenck reports the first full-scale use of this method. Employing an axis rotation which maximizes the correlation between factor saturation and criterion indices, he makes an attempt to identify the factor of neuroticism and its subcategories. It is here that one begins to raise questions about the author's presentation.

1. Having administered a battery of 76 tests, he selects 28 for factorial analysis. The basis of this choice is not made clear. Presumably, the number of subjects involved does not determine the choice since in three tests $N=38$, and in the remainder $N=96$. Nor does the choice seem to depend on the significance of the correlation ratios (pp. 127–150). One wonders what principle of elimination was used.

2. The selection of criterion groups is based upon psychiatric ratings with which Eysenck repeatedly finds fault until a positive attitude is useful to his argument. Thus " . . . psychiatric diagnosis is of doubtful validity and low reliability" (p. 33). Later, after a discussion of inconsistent inductee rejections on neuropsychiatric grounds, he says: "We are left with the unreliability of psychiatric assessment as the most probable cause" (p. 91). But, he then maintains, " . . . as we have shown in the preceding section, the validity of psychiatric ratings for future breakdown has been established; it would seem to follow that methods correlating highly with psychiatric assessment would also show a certain degree of validity" (p. 99). Practically all of the experiments which bolster his arguments for a neuroticism factor depend upon psychiatric diagnoses or ratings of severity of neurotic disorders (e.g., pp. 106 f.).

3. A similar tendency on the part of the author to favor expedient interpretations is exemplified by his variable interpretation of correlation coefficients. Thus, a correlation of $-.21$ between intelligence and neurotic tendencies is considered to be a "very small one" (p. 116) and it is interpreted as indicating no close relationship (p. 149). Nevertheless, a series of correlations between level of aspiration tests and examination scores "gave results in the expected direction," albeit they are $-.20$ and $-.11$ in one case, and $-.15$ and $-.10$ in another (p. 266).

There is little doubt that psychologists will follow Eysenck's logical reasoning about neuroticism, despite the lapses indicated. But the step which follows will separate the environmentalist sheep from the hereditarian goats. Employing Holzinger's h^2 as an estimate of the contribution of heredity to total variance, Eysenck concludes that neuroticism as a factor is a biological unit which is inherited as a whole (p. 187). The crux of his argument lies in the differences between identical and fraternal twins on tests which Eysenck employs to differentiate normals from neurotics. Although mean test scores are the same for the two types of twins, pairs of identical

twins are characterized by higher intracorrelations, and the group of identicals as a whole is characterized by greater variance.

One possible source of irritation to the reader should be mentioned here. Eysenck seems to have the happy faculty of anticipating objections to his theses, and the unhappy one of meeting these objections with a combination of affirmation and partial recant. The assumptions underlying the use of h^2 are probably not met, he says (p. 189), but they may err in the direction of *minimizing* the *true* contribution of heredity. On the same page, however, he points out the possibility of obtaining a *lower* h^2 value in another culture if the environmental factors of that culture were more pronounced. Presumably then, there might be a culture in which it could be "proved" that the total variance attributable to heredity is negligible.

Eysenck rejects the notion, attributed to Freud, that psychoticism is a part of a normal-neurotic-psychotic continuum. Although by criterion analysis he distinguishes between "psychopath" and "hysteria-anxiety" types of neuroticism, he finds little difference between manic-depressive and schizophrenic types of psychoticism. The reader may be inclined to wonder if the basic premises of criterion analysis are sound. Quite apart from Eysenck's conclusions, it is probably no more difficult to distinguish psychotic subtypes than it is neurotic subtypes. On a priori grounds one would expect psychiatric judgments to be at least equally valid in making these distinctions. Yet having employed the distinctions to create criterion groups, we are required to believe that the differences we "see" are not valid because test behaviors of the groups do not satisfy a statistical notion. In this instance, at least, we might conclude that criterion analysis is better used to discover what is not known than to corroborate what is.

Eysenck's book is an important one because of its generally convincing logic and its freshness of approach. These virtues may well operate to make it a new choice point in psychological history. Such importance makes it imperative to evaluate this factorial method in the light of our developmental frame of reference. Apart from the specific criticisms already made, two major points should be emphasized:

1. Criterion analysis presumably has the advantages of being a hypothetico-deductive process. Careful examination indicates, however, that the hypotheses are mainly validated internally, i.e., within the confines of the method itself. (See particularly the first full-scale application of the method described in Chapter 4.) The few exceptions in Chapter 7 notwithstanding, it fails to comply with the rigorous requirement of independent external validations.

2. Criterion analysis is admittedly elementaristic, and its sponsor is openly opposed to organismic interpretations of behavior or personality (pp. 277–285). Citing hand-picked experiments, he refutes the meaningfulness of gestalt qualities and substitutes for them scalar values on orthogonal dimensions. Nowhere is it clear how this index description of personality can help determine the dynamics—the causes of change—of behavior. Nowhere is it clear how measured body sway—a reliable index of "neuroticism"—will predict today's equanimity and tomorrow's moodiness. Eysenck himself cites the difficulties inherent in standardizing his factor-loaded tests:

" . . . whether the tester is a pretty young girl, a domineering male, or a mature, sympathetic woman—all these influences may, and in some cases do, affect a person's scores. They are difficult to control, particularly as the same stimulus—a pretty young girl as tester, for instance—may mean quite different things and arouse quite different emotions, in subjects differing in age, sex, and marital happiness" (p. 294).

He might well have added here that the same stimulus may be perceived differently by the

same subject from day to day or minute to minute. Not only the test, but the whole theoretical structure is thereby placed in jeopardy.

Eysenck—*The Structure of Human Personality*

In this latest book, we find neither essential changes nor further developments of Eysenck's theoretical position. It is useful primarily as a source book for those who would examine personality by the criterion of factor analysis. As such, it contains excellent summaries of factorial studies classified under the following chapter headings: "Analysis of Ratings," "Analysis of Questionnaires and Inventories," "Analysis of Objective Behavior Tests," "Analysis of Physique," "Analysis of Physiological Measures," "Analysis of Interests and Attitudes," "Analysis of Correlations between Persons," and "Analysis of 'Trait' Measurements."

Each chapter comprises a historical survey and a critical analysis of the area under consideration from the standpoint of its experimental worth, conceptualization, and the factor methods employed. For those who have read *The Scientific Study of Personality*, it goes without saying that the critique is made through no ordinary orthogonal eyes, but through maximally rotated Eysenckian ones. In this regard, it is regrettable that Eysenck does not maintain throughout the sense of humor which shows up so well in the Preface: "... it might be wise to follow psychoanalytic practice and lay it down that criticism of factor analysis should be confined to those who had themselves been factor analysed!" (p. xiv). We are treated, instead, to a steady display of the argumentative ingenuity and the perspicacity that are so characteristic of the author.

Although the book is intended in part "to bring together in one volume some of the major theories of personality organization . . . " (p. xiii), very little use is made of theories or experimental devices which do not imply either a factorial approach or a trait-type interpretation. Such organismic or "specificity" studies as do appear are employed mainly as foils for a dimensional point of view.

The preliminary anticipation—and the concluding assertion—made by Eysenck is that an examination of the seemingly contradictory data in factorial explorations of personality actually demonstrates a basic agreement. The evidence for many different personality dimensions is traceable mainly to semantic disagreements, inadequacies of the factor techniques employed, and the differences in aims of respective investigators. That these differences are peripheral is made clear by reference to a hierarchical structure of personality organization—from specific responses, through habitual response and trait levels, to a type level. At this highest organizational (type) level, he believes that three orthogonally related dimensions are implicitly agreed upon—Neuroticism, Extraversion-Introversion, and Psychoticism. Eysenck does not imply that these are the only higher-order noncognitive factors. In fact, he states: "Nor can it be maintained that these are the only higher-order factors which may be discovered; nothing can be said yet about the total number of such factors required" (p. 319). Yet, curiously, in discussing second-order factors involved in social attitudes, he strongly denies the likelihood of their being more than the R (conservative-radical) and T (tough-tender minded or practical-theoretical) factors already determined (p. 244). Such

assurance seems out of keeping with the scientific spirit so readily invoked elsewhere by the author.

Eysenck's books, considered together, constitute an extraordinarily well-documented and tightly woven thesis. Although in reading them one encounters a surfeit of "factors" and "dimensions," there is no doubt that the internal reasoning is both challenging and sound. They suffer particularly from a paucity of "external" (e.g., anthropological) supportive material such as distinguished Cattell's (1) factorial approach to personality. For those who are factorially inclined, the system outlined may be completely acceptable as a pathway to personality. For others, its monorail construction may prove to be discouraging. Whatever his point of view, no psychologist can afford to ignore it.

Vernon—*Personality Tests and Assessments*

After the supercharged atmosphere engendered by the Eysenck books, this slim volume comes as a cool and refreshing breeze. It covers almost exactly the same ground as Eysenck's (1953) review of personality tests, but it does so without restricting the material to factorial studies exclusively. Although the book is only 200 pages long, it reports succinctly on attitude and interest measurement, ratings and questionnaires, physical and behavioral tests, and interview assessments. In addition, there is material on cognitive tests, and a chapter on projection (English nomenclature for the American "projective") techniques.

Vernon reports and interprets data in a calm and studied manner. Because of its brief but unhurried digestion of assessment devices and its cogent but dignified criticism, this book is recommended to the students and scholars alike who desire an unbiased orientation to the field of personality testing. It brings no solace to those who seek vindication of one or another technique, but treats all techniques as legitimately adventurous early steps in the learning-to-walk period of valid assessment.

Vernon's book is written from what he calls a "trait point of view," but not so rigidly as to make him an idol worshiper. He accurately points out the difficulty of selecting distinctive syndromes of behavior for criterion purposes, the unreliability of psychiatric and associates' ratings used for the same purpose, and the probable existence of traits which "cannot be anchored to any syndrome" (p. 11). Even more important, he recognizes the artificiality of studying personality in isolation from society.

Vernon's contentions, that " . . . an infinite number of solutions exist to any factorial problem" (p. 12) and that "no two leading psychologists agree as to which are the best dimensions" (p. 12), are in marked opposition to Eysenck's position on both points. So, too, is his belief that *Q*- and *P*-techniques are especially fruitful. The two authors do, however, find common ground in their agreement upon the subjectivity and unreliability of the "clinical" approaches. Eysenck would discard them entirely on this account, but Vernon argues for the use of objective tests and techniques to back them up or correct them.

Vernon's chapters follow a fairly consistent pattern of concisely reviewing subtechniques which fall under selected rubrics; each includes clearly labeled discussion sections when the author feels they are warranted. An example of this procedure, well worth reading, is the

discussion of paper-and-pencil personality tests in the chapter, "Self-Ratings and Personality Questionnaires" (pp. 136–140).

His eight-page concluding chapter, "Conclusions and Future Developments," is a masterpiece of fulsome brevity. It conveys neither the extent to which his summaries cut cleanly to the heart of the evaluation, nor the number of research programs implicitly designed. Its three sections deal with the situations in which personality assessments are principally required: selection, experimentation, and diagnosis or guidance. The use of personality tests in selection procedures is the most successful, Vernon feels, because theoretical problems of personality are at a minimum, and external validation criteria are readily available.

From the standpoint of diagnosis, guidance, treatment, and control, however, the reverse is true. Theoretical problems are maximal, and criteria—both internal and external—are generally lacking. For those who are familiar with Vernon as a protagonist of factorial and trait-composite research, his recognition that they are cross sectional, and not dynamic, may come as some surprise. Even more unexpected, perhaps, is his hope "that a more fruitful system will eventually replace [trait psychology]" (p. 205).

His final plea is expressed as a need for the direct recording of behavior. This, he feels, will reconcile trait psychology with field theory, with the study of social groups and processes, and with longitudinal studies of personality development.

Nowhere, perhaps, will the American psychologist be brought face to face with one of his own problems more clearly than in this book. His English colleagues have observed and have often pointed out the limitations of clinical techniques, such as interviews, Rorschachs, and ratings, when used as everyday diagnostic tools. Although the critical studies most often cited have been made in this country, they have had less influence here than abroad in actually changing practice. It appears to be difficult for at least two English psychologists of repute—Eysenck and Vernon—to understand how we can, at the same time, be scientifically self-critical and clinically self-indulgent. This trait composite has yet to be explored by the culture-oriented psychologist.

Spinley—*The Deprived and the Privileged*

Spinley's book serves to show how a good idea can go wrong. The idea itself is symptomatic of one probable trend that personality psychology can be expected to take in the future. Two hypotheses imply, roughly, the direction of this development:

1. Groups exist which can be distinguished by their material and social environments.
2. Over and above individual differences, there are "personality types" which can be associated with, and attributed to, specific cultures or subcultures.

Spinley would add to these a third hypothesis, that "there is a basic personality type; therefore the groups will possess some cultural aspects and some personality trends in common" (p. 18). Her book, a modification of her doctoral dissertation, attempts to substantiate these statements.

The populations chosen for experimental verification consisted of two English subcultures: one was a "slum group," the other a group of economically privileged graduates of English Public Schools. The author unfortunately obtained her data from the two groups in quite different

ways. Among the slum dwellers she played the role of participant observer in successive age-group clubs in a settlement house. In addition, she utilized structured interviews with informants selected from among social workers and mothers. The privileged group data consisted of 42 solicited life histories that were supplemented by interviews with nurses-in-training to establish optimal child-rearing practices. Rorschach tests were administered to samples of 30 people from each of the two subcultures.

One of the most informative chapters is that in which data from the deprived group are collated. A series of 117 items characterizing this group are arranged in maturational order, and each is coded according to its source: statements based on field observation and supported by the large majority of informants; statements based on field observation, but with the opinion of informants fairly evenly divided; statements based solely on observation; and statements based solely on data from informants. The data from the privileged groups are extracted from the life histories and hence do not necessarily overlap the statements about the deprived group. Nevertheless, the author derives a series of characteristics that purportedly typify the personalities within the respective groups. In armchair fashion she then attempts to reason out the culture-causal factors producing each. Finally, having administered and scored the Rorschachs herself, she finds by comparative analysis that her data are consistent in differentiating privileged from deprived personalities as she depicted them.

Spinley's book could well be labeled a study in unreliability. Neither the instruments nor the judgments are shown to be reliable. Her own reliability is not guaranteed, and her role introduces the dangers of halo effect and unintentional bias. It should be noted that Spinley is aware of these sources of methodological impurity, but pleads the limitations of time and resources of the lone investigator.

When objectives rather than methods, are considered, it would appear that Spinley in some respects had taken her cue from Vernon in looking for socially induced personality syndromes. She is, of course, not the first to do so, nor is her approach essentially different from the workaday methods of sociology and cultural anthropology. To the "personality psychologist," however, a change in objective might be desirable. Perhaps this could be an emphasis on socially induced *perceptual* prototypes rather than on socially induced behavioral prototypes. Better still, we might hope to trace the relationship between the objective social situation, the perception of that situation, and the behaviors which follow from it. In this way, we might crystallize the functional interplay of the many disciplines which study society and the person.

Notcutt—*Psychology of Personality*

Among the books reviewed here, this is the only one which expressly attempts to present the gamut of contemporary theories of personality, including the techniques used, the results obtained, and the applications thereof. The categorization of personality theories that Notcutt employs as a point of departure leads one to expect a thorough and uniform development of each, somewhat in the manner of Hilgard's treatment of learning theories. Instead, one finds a hodgepodge of historical reflections

interlarded with tabulations of basic motives, personality types, and defense mechanisms.

In twelve extremely short chapters Notcutt covers the history, interaction, and interpretation of seven theories, two major assessment methods, and the logic of validation. The coverage, as might be expected, is spotty and thin. The depth of treatment may be evaluated by quoting briefly:

> It is said that in some parts of the United States morticians employ minions to listen in to the short wave police radio, so that whenever a fatal accident is reported, they can hurry to the spot and sell a funeral. If psychologists had shown more enterprise of this kind, the subject might have progressed faster (p. 185).

In short, Notcutt's abortive analysis of personality theory is not recommended reading.

Nuttin—*Psychoanalysis and Personality*

In recent years there has been considerable conflict between psychoanalytic theory and certain religious beliefs. Nuttin's book has the distinction of being among the first to deal with this problem by reinterpreting and extending a psychoanalytic point of view, rather than merely opposing it.

Nuttin gives an unprejudiced account of Freudian theory in the first chapter, but gradually and gracefully weaves into it the points he is to use later in refuting its application to "normal" behavior dynamics. Thus, Freud failed to examine seriously the role of the conscious ego; Freudian theory is inadequately bolstered by experimental evidence; psychoanalytic *theory* is not inseparably connected with psychoanalytic *therapy*. Arguing that psychoanalysis is not the only psychotherapeutic method, Nuttin points to Rogerian nondirective therapy as a desirable alternative. This particular choice supports the position that such therapy "permits the patient to construct his own personality" (p. 91) and that it is based upon a "social" rather than a directive relationship.

Nuttin is now ready to reject unconscious motivation in favor of freedom of will as a major determinant of normal behavior. An interesting point in his thesis is that very little human behavior is ***not*** unconscious, whether it be of psychic or biological origin. What is crucial, he says, is not the source of needs, but the felt need and its influence. It follows from this, he continues, that the psychology of normal people need not be based upon repression but upon the law of effect. Thus normal behavior develops according to success achieved and consciously known. Undesirable behavior disappears because "it does not continue to bring satisfaction to the whole personality" (p. 188). Nuttin completes his argument with the postulation of three levels of human activity correlated with basic needs: a psychophysiological level, a psychosocial level, and a spiritual level. The last level is coordinated particularly to man's need for self-preservation and self-development.

Nuttin has evolved a theory of normal behavior to fill the gap presumably left by those concerned with pathology alone. In so doing, however, he creates a dichotomy which is untenable in psychological theorizing. His highly speculative and metaphysical reasoning is supported only by an occasional clinical interview. His conclusion is completely predictable. Nonetheless, his attempt to cope with, rather than oppose or proscribe, "controversial"

material is a refreshing change. It seems likely that Nuttin's book will find wide acceptance in those groups which need to reconcile psychotherapy and religious faith. Despite its persuasiveness, it is not likely to gain wide acceptance among personality theorists.

Jersild—*In Search of Self*

This book by Jersild—the first of the American authors represented here—is a remarkable reiteration of Nuttin's premise regarding the behavior of normal people. Nuttin argued that people operate in the direction of constructive self-realization, and achieve it through the operation of the law of effect. Jersild, influenced by Lecky (**3**) and Harry Stack Sullivan (**5**), holds similarly that "a person accepts and incorporates that which is congenial to the self-system already established, but he seeks to reject or avoid experiences or meanings of experiences which are uncongenial" (p. **14**). Unlike Nuttin, however, he believes that people may show either self-acceptance or self-rejection, the former being essential to mental health, the latter its unhealthy counterpart.

In Search of Self reports a fact-finding procedure designed to help teachers better understand their students and to assist them in achieving self-acceptance. It consists of a tabulation of items reported by students in written essays on "What I Like About Myself" and "What I Dislike About Myself." The responses of almost 3,000 individuals, ranging from fifth-graders to college seniors, are reported according to grade level, sex, and type of response. Unfortunately, statistical information is limited to frequency and percentages of individuals reporting in each category. Although the data actually suggest little more than that young people may like or dislike themselves for almost an infinity of reasons, Jersild builds precariously on this sparse evidence in his discussion of categories of self-acceptance and rejection. He could readily have made his appropriate and instructive remarks without reference to the research data. His most valid generalization, labeled the "universal language of the self," is the recognition that "categories of self-evaluation at any one age level are also prominent at other levels" (p. 30).

The contribution of Jersild's book may not be unique from the standpoint of personality theory, but it is certainly a timely and well-developed contribution to educational theory and practice. Part Three of the book is devoted to the problem which the school and teacher must both meet—an adequate setting for the individuality of student needs, with particular reference to self-acceptance. To this point, he says:

> The failures, reminders of limitations, and the rejection which children face at school are often artificial and forced. They may have the effect of humiliating the child by depreciating his worth in a manner that does no good to society and does him great harm. Much of the failure at school is contrived. Much of the depreciation children encounter there is based upon false evaluation (p. 91).

With regard to educational "devices," Jersild points out the uncertainty of their results. In group discussions, for example, anxiety may not be released, but by-passed in intellectual or logical flight. On the other hand, individuals may betray by their exaggerations, not so much their thoughts as their feelings on the subject under discussion. It becomes the job of the teacher to appreciate the individual effect and to shepherd the student's struggle to be himself. In so doing, the teacher will

himself gain opportunity for productive rather than conforming behavior. In addition, he will be better able to cope with his own problems, particularly those evoked by contact with students.

Finally, Jersild clarifies the position of the teacher as a psychologist. He is not, of course, to take on the role of the professionally trained psychologist or psychiatrist. However, "every teacher is in his own way a psychologist. Everything he does, says, or teaches has or could have a psychological impact. What he offers helps children discover their resources and limitations. He is the central figure in countless situations which can help the learner to realize and accept himself, or which may bring humiliation, shame, rejection, and self-disparagement" (p. 125).

In summary, Jersild's book has the virtue of attempting to translate personality theory into an action level. Those who operate in the rarefied atmosphere of theory for theory's sake may find it speculative and wandering at times, but will not reject it. On the other hand, the classroom teacher for whom it is surely intended and appropriate may find it too abstract and theoretical to follow. Perhaps its optimal place is in the hands of those who teach teachers, as a working guide to a sophisticated educational philosophy.

Harrower—*Appraising Personality*

This book was ostensibly written to acquaint the medical practitioner with the nature of psychological testing and its value as an adjunct to diagnosis and treatment. More obvious is its attempt to establish the respectability and validity of certain projective techniques, particularly in the hands of a trained psychological interpreter. The devices with which Harrower is concerned are the Szondi test, the Miale-Holsopple sentence completion test, human-figure drawing, Wechsler-Bellevue response "scatter," and the Rorschach test.

The method of presenting these devices is an unusual one—a not very Socratic dialogue between a physician and a clinical psychologist. The physician is no ordinary general practitioner, being much too well primed. He asks about the Szondi pictures, "What are these strange looking faces?" and later, "In what way may I be legitimately paranoid? What does it show if I like the paranoid faces?" The psychologist replies with characteristic clarity, "You would accept consciously the need to be emotionally driven beyond yourself, to become involved in or involved with other things or persons and by so doing extend your own frontiers and boundaries" (p. 126). It is unfortunate that instances of this type of dialogue occur all too frequently, the psychologist speaking with equal assurance on each such occasion. It is possible to summarize his confidence (Harrower identifies with a male psychologist in this book) on the basis of the statement " . . . by and large, psychological tests have the same margin of error as do those concerned with somatic phenomena." She is referring specifically to the 10 per cent of false positives on the Wassermann test, which was taken as a sample of somatic diagnostic techniques. It would seem that Harrower's interpretation of validation studies is at great variance with that of Vernon, Eysenck, and most American psychologists. While all might place credence in the ability of these devices to mirror some extremes of personality, their present determinable

accuracy for an unselected group would certainly be less than 90 or even 50 per cent.

Harrower does present one area with more restraint, viz., her interpretation of "scatter" on the Wechsler-Bellevue. Especially good is her review of responses to single test items, particularly with respect to the clinician's opportunity to make judgments by observing the patient in action. It seems quite likely that much more of the diagnostic success of these techniques is attributable to such observation than to the test indices and scores themselves. This point is corroborated in Harrower's (synthetic?) cases reported in testimonial fashion in the last section of her book. A number of physician referrals are seen by the psychologist, who reports back to the physician the psychological status of the patients. The first of these—under the title "Which Twin Has Epilepsy"—presents no problem if one ignores the test results but reads carefully the behavioral description. This reviewer, at least, was able to make an educated guess (a correct one), and presumes that with prodding he could also have told which twin had the Toni.

The next case deals with a "backward" girl, about whom the physician inquires, "Would you give me your opinion as to the chances of her success in a therapeutic venture?" The answer would be obvious to most who had determined her verbal IQ to be 50. With Harrower, it requires the combined virtues of Szondi and Rorschach tests to divulge a psychotic ego picture, and hence contraindicate therapy.

Psychologists of logical bent know that somewhere they must *hold* the line, theoretically speaking. This book, although certainly written from honest convictions, is apt to designate where they will *draw* the line, instead.

Blum—*Psychoanalytic Theories of Personality*

Blum's avowed goal is to supply a framework for psychoanalytic personality theory since it is at once the "most comprehensive one of its species" (p. viii), and has fared the best in practical applications. Although we may question these premises, it is apparent that the author has summarized exceedingly well the many and varied psychoanalytic points of view represented. Regrettably, the coverage is more extensive than intensive, and its structure encourages browsing curiosity rather than intellectual involvement.

Each chapter of the book represents a segment of the chronological sequence of personality formation, psychoanalytically conceived. It thus begins with "Prenatal and Birth Influences," includes the "Latency Period," and ends prematurely with "Adult Character Structure." Within each chapter, subsections are devoted to ego and superego formation, psychosexual development, relations with others, and mechanisms. The subsections are in turn subsected (an induced neologism) to note the positions taken by Freudians, neo-Freudians, and other psychoanalytic theorists. Each chapter ends with a summary and author's notes; the latter consist of reports of experimental research presumed to be related to the chapter material, suggestions for further research, and the author's critical comments.

The organizational structure of the book seems to defeat its purpose in a number of ways. The persons for whom the book was written—graduate and undergraduate students in

psychology and related disciplines—will not enjoy reading it through because of the difficulties of assimilation inherent in its discontinuity. If they attempt to trace a single concept such as "libido-regression," they will be forced to turn repeatedly to the index and to the respective sections where the term occurs. To compare one such concept with another makes the process doubly inconvenient.

For research workers, the structure is equally hampering. Blum, in his notes on each chapter, summarizes a variety of recent psychoanalytic research materials. The summaries, however, are so embedded in other material and so far removed from the theoretical content to which they refer that they appear to be superficial and unnecessary. The major accommodation for the research worker is actually the 295-item bibliography.

Patty and Johnson—*Personality and Adjustment*

Any textbook on personality must comply with unusual demands. It must not be too lofty, erudite, or theoretical—nor too elementary, vague, or contradictory. It must give the essentials of theories without vitiating them, and it must do so in readable fashion. It must, above all, enable the student to see the relatedness of theories to the people from whom ultimately they are derived and to whom they will apply.

Patty and Johnson fall far short of these utopian goals and some lesser ones. At first glance, the Table of Contents gives promise of a well-structured approach to mental hygiene, using personality theory as a vehicle. But the development of the latter to this end is certain to be meaningless or misleading to the student. An example of this comes early in the chapter on motivation, where Lewin's topology is introduced:

> This viewpoint made extensive use of *field theories*, by means of charts showing arrows (vectors) symbolizing "behavior tendencies" or "pressures." . . . It stresses the fact that animal behavior seems to maintain a sense of direction in a complex situation. Its weakness lies in the difficulty, if not the impossibility, of describing behavior in sufficient detail by means of the available symbols (pp. 49 ff.).

It is a rare student who will derive from this any concept of Lewin's dynamics, or do more than reject them. It would be far better to have left it unsaid.

The frequency of misconceptions and misdirections is so great as to make recasting unbelievably difficult for the instructor and impossible for the student. The chapter on perception, a keystone in the author's development of behavior determinants, consists of exactly two topics: illusions and "set," and projective techniques. Chapter 7 contains this unverifiable datum:

> It appears that mental ill-health in the expectant mother affects the development of the child in ways which may appear later as anemia, nervousness, underweight, and other equally general conditions (p. 160).

There is little use in multiplying these instances, which are primarily supplied to let the reader make his choice of text both forewarned and forearmed.

Epilogue

In these last remarks, I should like to claim the privilege of forsaking the editorial third-person and give my pen a reprieve from conventional expression.

The lot of the reviewer, like that of the policeman, is not a happy one. When he wants to say, belligerently, that the author tried to stuff a

rhinoceros down a mouse hole, he feels restrained by the knowledge that those who criticize reviewers are rougher than those who criticize books. If he attempts a bit of whimsy, he may be accused of being precious or puckish. If he's for something, he's really against someone. It is probably thus that reviewers develop tough hides that cover once tender hearts.

In looking back on these reviews, I am struck by their general negativity. I do not think that this comes from looking too long through jaundiced eyes. I am inclined to believe that for books on personality the year has indeed been fallow and lacking in conceptual breadth. But Pollyanna-like, I feel that it represents only a breathing spell, a period in which we consolidate our gains and prepare to strike out in new directions. It was for this reason that I characterized our psychological course as epicycloidal, and our position as nodal. It is fortunate that the analogy stops here—for the epicycle comes back to its starting point, while we may, with scientific outlook, hope to approximate true rather than apparent motion.

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BUROS, OSCAR KRISEN. (Ed.) *The fourth mental measurements yearbook.* Highland Park, N. J.: Gryphon Press, 1953. Pp. xxiv +1163. $18.00.

Oscar Krisen Buros' *The Fourth Mental Measurements Yearbook* is the seventh in a series of publications intended to make available to practitioners in psychology, education, and related disciplines the rich resources of tests and measurements. The series began modestly with the little pamphlet "Educational, Psychological, and Personality Tests of 1933 and 1934" (issued in 1935), followed by a cumulated monograph "Educational, Psychological and Personality Tests of 1933, 1934 and 1935" (issued in 1936). A year later, Buros published "Educational, Psychological and Personality Tests of 1936: Including a Bibliography and Book Review Digest of Measurement Books and Monographs of 1933–36" which, however, abandoned the notion of cumulating the test entries. In 1938 was published, in book form, *The Nineteen Thirty Eight Mental Measurements Yearbook of the School of Education, Rutgers University*, followed in 1941, by *The Nineteen Forty Mental Measurements Yearbook*, and in 1949 by *The Third Mental Measurements Yearbook.*

The current volume is a beautifully printed and well-organized tome of more than 1,100 pages. In no way can it be compared with the first little pamphlet. The earliest three publications were primarily test bibliographies. The shape of things to come was first evident in the 1938 publication. For in that volume, Buros undertook the prodigious tasks of getting cooperative reviews of tests (issued as separates in the English-speaking countries) and of compiling significant excerpts from published reviews of books and monographs related to tests and measurements. In 1940, he made this task his by tradition; in the present volume, he has dignified the tradition by an opulently produced book.

The 1953 volume is intended to be a bibliography as well as a review of tests published in the period 1948–1951. But Buros has continued the practice of adding to the bibliography previously unlisted items and, whenever possible, reviews of such tests published since the series began in 1933; the scope is considerably greater in the current volume, which lists, for the first time, restricted tests together with reviews of a few of them. Indeed, Buros, as well as the test organizations, is to be congratulated for removing some of the so-called secrecy from the instruments utilized by agencies like the College Entrance Examination Board, the Educational Testing Service, and the National League for Nursing Education. Further, the volume continues the practice of listing and giving excerpts from books and monographs related to tests and measurements for the period covered, 1948–1951.

In a sense, Buros' publications represent a compromise. In the early thirties he intended to develop a test-consumers' research organization. Failing to realize such an accomplishment, he devoted himself to the lesser, but very important, goal of supplying to all, particularly to test consumers, "frankly critical re-

views." Such a limitation, of course, meant that he had to become dependent on outside reviewers rather than upon members of his own research staff. During the years, Buros has had the cooperation of the leading specialists in test construction and test utilization. Nor has Buros left the task to them alone; he has supplied the bibliography of published research related to each listed test. Such cumulation of related material has been, and should continue to be, a valuable resource for the student and the scholar. For example, the cross-references for the Wechsler-Bellevue Intelligence Scale now goes to 312; and for Rorschach to 1,219. As for books about measurement, the review excerpts that he has edited and compiled make another basic contribution toward appreciating the growing sophistication of test constructors.

An editor who conceives his function to be the improvement of test making and hence the improvement of test using deserves well of all who make and use tests. Indeed it is fitting that the Division on Evaluation and Measurement of the American Psychological Association and the Psychometric Society jointly expressed their appreciation to Buros for his enterprise over the past 20 years. The fact that these two organizations memorialized Buros indicates the value and significance of the work. Indeed, the volume's deficiencies seem entirely attributable to Buros' limited financial resources rather than to any administrative or intellectual constraints. The volume, in so far as it is not cumulative, makes it necessary for the interested user to manipulate several volumes, for test reviews are to be found in the four volumes for 1938, 1940, 1949, and 1953. Perhaps the time has now come when test listing and test reviewing can be done best through the medium of an official magazine sponsored by the American Psychological Association. Such a publication would give greater currency to the newer tests faster. With the expanding test field, it seems too long to have to wait four or five years for Buros' next volume. Indeed, if subscribers are willing to pay $18 every four or five years, there may be even more who can afford $4 or $5 every year for more current material.

Buros, in his attempt to be fair to every test maker, has "leaned over backwards." At one time, he forwarded the critical reviews to the test authors and/or publishers to make certain that the critiques were related to the facts. This apparently was so formidable a task that he abandoned it in favor of trying to achieve at least three reviews of each test by different analysts. Fair as that may be, it seems not nearly as valuable an objective as getting at least *one* review of every listed test. In a random sample of 20 entries, four were not tests at all (a manual on aphasia, a book on Rorschach, a manual for driver selection, a series of forms for employee selection); seven were of new tests not previously listed, but which remained *without* review; three were for new tests not previously listed with a review; four were for reviews of a revised or supplemented test; and two were reviews for tests which had been previously listed and reviewed. Perhaps editorial energies could have been expended to get all the new entries reviewed at least once.

It is not difficult, however, to recognize that the reviewers themselves frequently add their personal prejudices to their test appraisals. Sometimes these biases are minor ones, such as criticizing the directions,

sometimes they are major ones, such as questioning the use of primary batteries of standardized achievement tests. For one reasonably familiar with the proclivities of the individual reviewers, it is quite a confirming reaction to be able to designate the reviewer just from the review. In a field as diverse as testing, various specialists have developed special attitudes: e.g., criticism of objective scoring as restricting the scope of testing, or the impossibility of getting truly representative national norms, or the overclaims on diagnostic profiles. As is inevitable, the reviews are uneven because of the very specialization of the reviewers. In one sense, the user of the volume should have available specifications of "all the players."

Nevertheless, most of the advice is significant and cogent. A few quotes give the flavor of the volume: the test has "little to recommend it . . . in preference to many similar commercial tests on the market," or "the items in the test also have a wide range of difficulty, the earlier items being very easy; but the last, even with the answer key available, gave the reviewer considerable difficulty," or "among the major ones are: insufficient data about *what* it predicts, temptation to do elaborate pattern analyses on subjects' scores, some urban-rural inequity in the standardization sample . . . " or " . . . strong relation to the . . . [somebody else's] reading tests in form, vocabulary, and general testing procedure; and the first grade tests could well be additional forms of the [somebody else's] *Tests*," or "study of the items reveals considerable dependence upon recall of factual information. . . . " Future editions of the volume will profit from attempts to classify the *relevant* information by validity, reliability, sampling for normative data, character of norms, relevance for specified objectives of appraisal or of instruction.

It is the lack of a comprehensive plan for coverage that makes for the primary difficulty with the reviews: they tend to be so prolix and discursive as to omit much information that a consumer should have for his decisions. Certainly, the bulk of the reviews fails significantly to meet Buros' requisite: "Reviews should be written primarily for the rank and file of test users." Too often, the reviews seem to be prepared for the reviewer's peers.

The current reviews, however, occasionally give evidence that earlier reviews have led the authors to make improvements in subsequent revisions of their tests; unfortunately, in too many instances, the reviews give contrary evidence that the test authors were immune to such analyses. Over the 20 years, however, Buros has raised the standards for the technical aspects of test making. It is hoped that a magazine may continue these important reviews to make them more up to date and available at a price that will allow the rank and file to use them more freely. I know of no one as capable as Buros to be its editor.

IRVING LORGE
Institute of Psychological Research
Teachers College, Columbia University

MOWRER, O. H., and 21 Contributors. *Psychotherapy, theory and research.* New York: Ronald Press, 1952. Pp. xviii + 700. $10.00.

This is an important book, but, by virtue of the range of content and number of authors represented, it is a difficult one to review. Part I, entitled "Theory, Concepts and Appli-

cations," consists of eight chapters, two by Mowrer and six by other contributors. Part II is entitled "Research Methods and Results" and consists of 11 chapters. Of these, Mowrer is the author of two and coauthor of three more; the remaining six chapters represent the work of other contributors. Mowrer also provides a brief Preface and Introduction; there is, however, no attempt to summarize or integrate the many diverse contributions, which vary widely in both content and quality. All but two of the 19 chapters were written expressly for the present volume. Finally, the book is big: the 700 pages average about 500 words per page and include 21 tables, 120 figures, 211 footnotes, and a bibliography of over 300 items.

In his Preface, Mowrer states that "mental disorder and the challenge of its more effective understanding, treatment and prevention constitute one of the major preoccupations of our contemporary civilization." He observes, however, that only since World War II have psychologists concerned themselves centrally with psychopathology and psychotherapy. This volume, written exclusively by psychologists who for the most part are experienced therapists as well as active investigators, is presented as a partial report of progress regarding developments on this research frontier. Because of this general limitation to authors engaged in actual research on psychotherapy, the volume is in no sense representative of either the theory or practice of present-day psychotherapy. It does, in the 500 pages of Part II, include a good sampling of the research methods used and the preliminary results obtained by psychologists doing research in psychotherapy.

Part I begins with a delightful chapter by Rollo May tracing the historical development of social attitudes toward neurotic manifestations and the modes of treatment provided at different historical periods. This is followed by an admittedly speculative chapter by Carl Rogers in which he emphasizes the importance of examining the process of psychotherapy entirely apart from its utility, and after conceding "that the process, directions, and end points of therapy may differ in different therapeutic orientations," attempts to delineate the major common aspects of the experiences of clients undergoing nondirective therapy. In the third chapter, Mowrer undertakes to develop briefly a systematic account of both "neurosis and psychotherapy as interpersonal processes." In this paper, Mowrer continues his argument with more orthodox psychoanalytic theorists on the question of the nature of repression and its implications for therapeutic strategy in handling transference.

Chapter 4 by William Perry and Stanley Estes represents an attempt to resolve the directive-nondirective controversy by conceptualizing the counseling process as one requiring "The Collaboration of Client and Counselor"; the counselor plays two distinctly different roles in two phases of the counseling process. In the first, he should be truly nondirective, but after the client accepts responsibility for his own active participation in solving his problems, the counselor may appropriately change his role to that of an expert collaborator in the actual process of problem solving. Preliminary research findings are presented to support the validity of this orientation.

Chapters 5 and 6 are both analyses of psychotherapy as a learning process. In the former E. J. Shoben

suggests that in therapy the patient learns a new "comfort reaction" to replace his anxiety reaction and that these new reactions generalize sufficiently to extraclinical situations to permit the patient to develop new and more appropriate instrumental social acts. In the latter, Mowrer defends his modifications of Freudian theory on the basis of his two-factor theory of learning.

Chapter 7 is a somewhat modified version of Milton Wexler's previously published interesting account of his two years of therapy with a severely disturbed schizophrenic patient and a statement of the modifications in psychoanalytic treatment which he believes necessary for successful therapy with such patients. In general, his suggestions for revision of theory and practice seem congruent with those proposed by Mowrer for the treatment of neurotics, which perhaps accounts for the inclusion of this paper on schizophrenia in a book otherwise dealing exclusively with neurotic rather than psychotic behavior.

Chapter 8 is a discussion of the implications of psychotherapy for public health, by Joseph Bobbitt and John Clausen, with especial emphasis on the parallels between psychotherapy, group activities, and other educational methods which show some promise of being effective both therapeutically and prophylactically in the mental health field.

Looking next at Part II, we find first a brief but useful summary by Jules Seeman and Nathaniel Raskin of the research methods and results growing out of "client-centered research on therapy." Chapter 10 is a reprint of Dollard and Mowrer's 1947 paper describing the Discomfort-Relief quotient as a method of measuring tension in written documents; this serves as an introduction to Chapter 11 (Mowrer, J. McV. Hunt, and L. S. Kogan) summarizing further studies utilizing the DRQ; it appears to be significantly related to therapists' estimates of therapeutic success and several objective measures of personal change during therapy. In Chapter 12, Fred Fiedler presents an integrated summary of his studies on the attitudes and feelings of therapists of different theoretical persuasions and the role of these feelings in the therapeutic relationship.

The next four chapters deal with the application of relatively new statistical procedures to clinical research. Chapter 14, written by Mowrer, is entitled "Q-Technique—Description, History and Critique." In this reviewer's opinion, this scholarly 60-page methodological monograph is the most valuable contribution of the entire volume. In it, Mowrer begins by illustrating recent applications of the technique in research on psychotherapy, traces the history of the technique and its relation to conventional factor analysis and other alternative research designs, and makes a clear-cut distinction between Q technique as a method of data collection (which he proposes to call S technique) and as a method of data analysis. Although agreeing that these "statistics of the individual" hold much promise for research on psychotherapy, he also reminds us of their limitations and pitfalls, unless used in the context of the alternate analytic procedures suggested by Cattell's "covariation chart" and by factorial design, in which case there seems to be real hope of integrating "experimental" and "clinical" types of research.

In Chapter 14, Lee Cronbach contributes a briefer and more general discussion of "Correlations between

Persons as a Research Tool" and its relations to other methods of analyzing complex data. Chapter 15, by Lester Luborsky, describes the "P" techniques (correlation of repetitive measures of the same person) and demonstrates its use with a patient in the course of psychotherapy. In Chapter 16, L. L. McQuitty describes still additional methods of evaluating "Personality Integration" and suggests their potential utility in clinical research.

Chapter 17 is another 80-page "monograph" by Mowrer. In this he first develops the thesis that neurosis may usefully be regarded as a "disturbance in communication" and that psychotherapy is essentially a matter of assisting the patient in re-establishing good channels of communication between different aspects of himself and with others. He then reviews an impressive series of empirical studies relative to language, psychopathology, and psychotherapy and suggests the use of several procedures (e.g., Osgood's "semantic differential") as promising methods for research on therapeutic change.

Chapter 18 is an even larger chapter —nearly 100 pages—entitled "Tension Changes during Therapy with Special Reference to Resistance," by Mowrer and three of his students (B. H. Light, Zella Luria, and Marjorie Zeleny). In the theoretical part of the chapter, the authors begin by reviewing the Freudian formulations regarding resistance and attempt a resolution of certain alleged "paradoxes." They next review and evaluate Rogers' views on resistance and conclude that the phenomenon is of sufficient importance both theoretically and practically to justify the development of methods permitting its objective measurement during the course of therapy. The major portion of the chapter is devoted to reporting the development of a simple and practical technique of measuring "palmar sweating" and enough preliminary data to indicate its promise as an index of tension and hence, perhaps, of resistance. The final brief chapter, written by L. I. O'Kelly, is devoted to a survey of the possibilities and limitations of other physiological measures as indices of change in therapy.

This then is the content of the book. It reflects a tremendous amount and variety of activity on the part of a great many psychologists. And, although the volume is heavily weighted with the thinking and research methods of Mowrer and his colleagues, a reasonable effort was made to include at least samples of research by persons with other orientations. This together with the relatively undeveloped and unorganized state of knowledge in the field resulted in a volume perhaps better characterized as a "Contributions to . . . " or "Readings in . . . " than a "textbook" or "treatise" as its title tends to imply. Certain of the chapters might, in this reviewer's judgment, have been better published as separates since they bear but little relation to the major theme of the volume. An integrative summary chapter would have been helpful, but perhaps unnecessary, to emphasize the fact that we are not yet ready for such an accounting.

The major criticisms that may be made of this volume are those which are inherent in the present undeveloped state of an art which may sometime become an applied science. One finishes the book with such questions as: "What is psychotherapy anyhow? "Is it effective—and if so under what conditions and with what kinds of clients or patients?" Even when re-

search results are presented, the reader has difficulty in evaluating them because our present state of knowledge does not permit an adequate description of either the nature or intensity of the presenting problem. Furthermore, in view of the extremely wide variety of activities subsumed under the rubric "psychotherapy," the reader is at a loss (unless he can read an entire series of transcripts) to know what really occurred in the treatment hours. Finally, as this book so well shows, the problems of developing criteria are most complex. For the most part, the authors of these contributions are fully cognizant of these major problems but confident that they are amenable to research.

Whether or not the reader agrees with Mowrer's theoretical orientation, he will find this a challenging and useful series of contributions to an increasingly important topic in psychology. It is a book which no one planning any kind of research on any aspect of psychotherapy dares to overlook. I predict that but few persons will read all the chapters in sequence, but that equally few will fail to find several of the chapters extremely valuable.

E. LOWELL KELLY
University of Michigan

LACEY, OLIVER L. *Statistical methods in experimentation.* New York: Macmillan, 1953. Pp. xi+249. $4.50.

LINDQUIST, E. F. *Design and analysis of experiments in psychology and education.* New York: Houghton Mifflin, 1953. Pp. xix+393. $6.50.

WALKER, H. M., & LEV, J. *Statistical inference.* New York: Holt, 1953. Pp. xi+510. $6.25.

Depending on one's point of view, it may be either distressing or satisfying to note the increasing extent to which training in statistical methods has invaded the curricula of psychologists in recent years. Little more than a decade ago the typical Ph.D. in psychology was generally considered to have a respectable statistical repertoire if in addition to histograms and polygons he had acquired an assortment of measures of central tendency and variability, manipulations of the normal curve, linear correlation and regression, and the critical ratio. Sophisticated indeed were students who had mastered the intricacies of the *t* test, chi square, and partial-multiple correlations. With due respect to Truman Kelley, the beginning of the modern era, characterized by the continuous translation of newer statistical logic and procedures for the consumption of mathematically naive but eager psychologists, can probably be dated to 1940, when Lindquist's *Statistical Analysis in Educational Research* and Peters and Van Voorhis' *Statistical Procedures and Their Mathematical Bases* made their appearance.

Since 1940, and especially in the last five years, a number of "advanced" statistical texts designed for psychologists have been published, and there has been a remarkable increase in the number of statistical articles and reports of complexly designed research studies. Also, the number of statistics courses offered in many graduate schools has shown a concomitant rise. In some schools emphasis on statistics has become so strong that students are urged to enroll in courses in mathematical statistics and spend summers at statistical institutes.

Thus, today the research psychologist is expected to possess a competence in statistical design, analysis,

and inference which can be produced only by devoting a substantial portion of the graduate program to courses in statistics or by motivating students to do a considerable amount of self-study. Perhaps unfortunately, the increased emphasis on statistics seems in part to have been accompanied by a decreased emphasis on psychophysics and measurement theory. If psychologists should also be competent applied statisticians—and this is debatable in view of the trend toward teamwork in research—it would seem that a partial solution to the problem lies in selling the idea that students should acquire more relevant mathematics at the undergraduate level rather than simply increasing the statistical load on graduate students.

Lacey's book represents an attempt to bring together the basic concepts and techniques of modern statistical inference in a form suitable for the first course in statistics. The main trouble with this excellent little book is that it is so completely introductory. Beyond a compact discussion of the rationale of experimental design and inference, the specific topics touched upon are the binomial expansion, the normal curve, the t test, chi square, simple correlation and regression, and the elements of fiducial limits. It is hard to judge where this book might fit into the program for the training of psychologists. The material appears to be too scanty and pitched too low for a full-fledged graduate course. However, at the undergraduate level one might venture to replace the usual semester in reductive and descriptive statistics with a course focusing immediately on statistical inference.

Perhaps the main utility of Lacey's book will be to serve as a reference text for courses in experimental psychology. In view of the current status of psychological research design, one can seriously question Lacey's claim that "when you complete the text, you should be able to read and understand over 80 per cent of the experimental articles in your field, and should also be capable of designing and executing many of the experiments which you yourself may wish to try" (p. viii).

Lindquist's book furnishes an amazingly detailed account of both the theory—reduced to algebra—and the principal applications of analysis of variance and covariance in educational-psychological research. Few psychologists will complain of a paucity of material in this book, which attempts to do for psychological experimentation what Cochran and Cox's *Experimental Designs* has done for biology and agriculture. Closely written but at the same time deliberately repetitive in the interest of good pedagogy, loaded with algebraic proofs and discussion of assumptions and pitfalls, illustrated with an abundance of actual experimental situations in both example and exercise, this book should make a major contribution to the quality of psychological research. There is easily sufficient material for a year's course in experimental design and analysis. Equally important, this will be a valued handbook for anyone engaged in psychological experimentation.

Although the entire book is unique in view of its exhaustive treatment of a multiplicity of useful designs, especially noteworthy are the chapters devoted to groups-within-treatments designs, controlling individual differences in factorial experiments through the use of "mixed" designs, and tests concerned with trend. The material on interpretation of interactions and the selection of valid error terms is

developed with outstanding skill.

This comprehensive and scholarly book would have been even better if a more adequate job had been done in referring to the relevant literature. The book is so self-contained that it tends to become dogmatic about a number of controversial issues. No book of this type should fail to convey the important concept that it is, after all, only a selective progress report on the never-ending search for mathematical models that approximate the relationships of observed data. This reader would gladly have sacrificed a number of the proofs in favor of a discussion of the problems encountered in dealing with non-orthogonal data. Although the section on Type II errors was admirably presented, it was a surprise to read the statement saying that the basis for testing Type II errors has been worked out only for simple randomized experiments involving two treatments (p. 71).

It was real joy to note the appearance of the long-awaited book by Walker and Lev. This book is a major contribution to the statistical training of psychologists. Just as Lindquist's book is unique in its treatment of analysis of variance, Walker and Lev have done a unique job in surveying the total field of statistical techniques useful to psychologists. In contrast to Lindquist's decision, the authors in this case make no attempt to supply "proofs" for procedures, but content themselves with ample discussion of the logic of underlying assumptions. The bibliographies at the end of each chapter are excellent, and the text and exercises provide strong motivation for follow-up of the literature cited.

Here again is sufficient material for at least a full year's graduate course beyond the introductory level. The combination of Walker and Lev with Lindquist might conceivably be compressed into a minimum of three semesters. Interestingly, Walker and Lev provide considerable material in the area of analysis of variance and covariance which is not presented by Lindquist.

Mere citation of the headings of the 18 chapters in Walker and Lev's book would, with the exception of the final chapter on "Non-Parametric Methods" written by Lincoln Moses, give little hint of its comprehensive coverage. Traditional material is presented in the context of the most modern of statistical logic, but in addition every chapter contains the bonus of an excellent selection of recently developed techniques for dealing with common problems that arise in the analysis and interpretation of experimental results. An extremely useful set of 24 tables and a glossary of symbols are appended.

In order to fulfill his function as critic, the reviewer feels obliged to make at least one criticism of Walker and Lev's book. In the section dealing with the comparison of two proportions based on the same individuals (p. 102), it would have been most appropriate if the reference had included McNemar's 1947 article as well as the article by Cochran published in 1950.

It cannot be stressed too strongly that the texts by Lindquist and by Walker and Lev are by far the best ones available in their respective areas of coverage. Both are based on the assumption that a statistical text for graduate students should not be a mere cookbook devised for spoon-feeding. Obviously these authors believe that research workers in psychology and education should be competent applied statisticians. The

reviewer concurs and hopes that teachers of statistics will have the courage to adopt these two books widely so that potential research will benefit accordingly.

LEONARD S. KOGAN
Institute of Welfare Research, Community Service Society of New York

LEHMAN, HARVEY C. *Age and achievement.* (Published for the American Philosophical Society.) Princeton: Princeton Univer. Press, 1953. Pp. xi+359. $7.50.

Lehman's topic is one of urgent concern to psychology and to society in general. Since age is an inescapable attribute of every person, it has been an ever-present vector in determining human behavior. In view of the fact that human longevity has greatly increased in the recent past and appears likely to increase further in the near future, better knowledge of the role of age has become progressively more critical.

Age and Achievement assembles in convenient form data which Lehman and his collaborators have been collating for more than two decades. Nearly all of Lehman's previous contributions apparently have been incorporated here, although the volume makes no explicit statement on this point and provides no complete bibliography of Lehman's publications. The book consists of earlier research reports partially rewritten for this presentation and supplemented by some new data.

The majority of Lehman's researches have been directed to the production of creative works, such as paintings, poems, musical compositions, inventions, and scientific discoveries, as a function of age. His general finding is that while masterpieces and "best works" may be produced at any time during the mature years, in many fields the peak of productivity is reached in the thirties and forties, if not before 30. The number of contributions per decade per surviving contributor is greatly reduced in the later years. Thus, the period between ages 50 and 70 has been responsible for a very small part of the world's best work, except in certain fields such as biography and history. During the fifties the production of "best works" per living creative worker in many specialties is only 20 per cent or less of what it was in the thirties, and later productivity is even less. If we extrapolate from Lehman's data, it appears that the imminent increase in the average life span from age 70 to age 80 or beyond will result in an inconsequential increase in creative contributions of first-rate importance.

These findings, if taken at their face value, are of great importance, and careful attention should be given to their interpretation. Most of the remainder of this review therefore will be devoted to an examination of this question. No lack of confidence in the accuracy of Lehman's data is implied. His data seem to us to be impressive in their objectivity, their quantity, and their self-consistency. We are concerned solely with interpretation. Lehman himself is most cautious in this regard; it is hoped that others will be equally restrained in regard to their generalizations.

First of all, it is necessary to keep in mind that nearly all of Lehman's studies of creative contributions are concerned not with the aggregate of creative products but only with "best works" and "masterpieces." This fact introduces into Lehman's studies inherent limitations which should be recognized. By definition no man can produce more than one "best work." If this product appears early in the

life span, any number of later contributions of nearly equal merit will not affect analyses such as Lehman's regardless of longevity. The comparative frequency of masterpieces at different ages therefore probably provides a very inaccurate index of the relative value of work done in different decades. Lehman's studies may show that man's "best" work is frequently done fairly early in life, but the relative value of other work is unassessed.

Lehman's finding that masterpieces in a given field are often produced in the thirties may properly be likened to a report by an anatomist or physical anthropologist that the maximum height of man is most often reached during this decade. Such a report, while interesting, would give us no measure of the degree of decrease in height which occurs in the later decades. It is true that Lehman's data show that older men produce fewer works reported to be of high value than do men in their thirties. But whether the value of the septuagenarian as a creative worker relative to the 30-year-old is 99 or 50 or 10 per cent cannot be determined by studying masterpieces.

Although Lehman's primary concern is with the relative frequency of the notable contributions at different ages, in a few instances he presents data on the *total* contributions, regardless of value, made in different decades of life. In contrast to the "best" contributions, the total numbers of creative works show little decline with age. This difference between the age trend for total productivity and the age trend for highest quality productivity is clearly one that deserves additional analysis in the future.

In selecting "best works" for study, Lehman has carefully avoided the introduction of any bias of his own into his data. Lists of the outstanding achievements in any field have, in each case, been made by others, not by Lehman. But this procedure, while commendable, does not avoid biases which may have been present in the evaluations of anthologists and historians, whose decisions are utilized by Lehman. Thus, if anthologists tend to choose the earlier of two works of almost equal merit produced by the same creative worker, this tendency would obviously produce an overestimation of the productivity of masterpieces in the early decades of life. We have no evidence concerning the presence or absence of a bias favoring selection of early works on the part of editors, compilers, biographers, etc.

Some of the anthologies and histories used by Lehman as sources of data were published shortly after the death of many of the men whose works are evaluated therein. There exists the possibility that historians and editors may hesitate to include the later works of a recently deceased man because of the difficulty of judging recent contributions. It is our impression that hesitancy in making judgments concerning the recent past is common among historians. If this is true, it would lead to low scores in later maturity for the men who died only a few decades prior to the publication of a source book or history.

In some analyses Lehman treats separately men of former centuries and men who lived recently. Men of former centuries show less decrement with age than do recent men. This fact is susceptible to the interpretation that there is a bias against the citation of a man's later contributions, on the part of authors writing soon after his death.

Further influences to be considered in interpreting Lehman's data are the possible changes in standards of com-

parison that may take place in the course of a life span. Each successive quarter of a century within the past 200 years has witnessed a great increase in the ranks of creative workers and in the number of entries in lists of notable contributions. This means that a man in recent centuries has had many more competitors for honors in his field at age 75 than he had at age 25. For example, in our own field of psychology, a man publishing today who wishes his work to be noted is potentially competing with a large proportion of more than 11,000 other psychologists. Had he been writing 50 years ago, he would have been competing with a total population of scarcely more than 100. A comparable change has occurred in most other fields. As suggested above, this great increase in number of workers introduces the possibility that the standards for "best works" in various fields have been progressively raised. Lehman's data may reflect not age changes but, instead, changes in the requirements for recognition that have occurred in the life span. Analysis of data for an era not characterized by an increase in the number of creative workers might give quite different "age" trends. Evidence on this point is lacking and, indeed, may be impossible to obtain. But the absence of such evidence should make us wary of interpreting Lehman's curves as representations of changes in the individual during the age continuum.

We have noted previously that Lehman presents data showing that scientists, inventors, authors, and other creative men born a century or more ago yield age curves, in regard to "important" works, somewhat different from the age curves produced by men born more recently. In general, the men of earlier centuries maintained a better record in later maturity than have men born more recently. Several interpretations are possible. One was suggested earlier in this review. Another is that standards of comparison in former times changed less drastically during the course of a man's lifetime than they do today.

Lehman has done a major service to psychology by assembling a multitude of facts relative to age and achievement. There are limitations inherent in his data, and we have indicated factors other than age which may be responsible in part, or in toto, for his "age" curves. Nevertheless, he has supplied the data which are fundamental to further studies. As is usual with those who pioneer in new fields, Lehman has posed more problems than he has solved. Lehman's own discussions, as well as the fine foreword by Terman, provide an excellent précis of the further research problems in regard to age and achievement which await solution.

WAYNE DENNIS
Brooklyn College

GRAY, J. STANLEY. *Psychology in industry.* New York: McGraw-Hill, 1952. Pp. vii+401. $5.00.

There has been a tendency in the past few years to rush the publication of textbooks in industrial psychology. This is true of texts in readings in industrial psychology as well as of those dealing with human and industrial relations. As a result, the selection of topics and the thoroughness with which they have been written are somewhat haphazard. Gray's text is no exception.

The book is written in a strictly logical order. In Chapter 1, the author introduces the reader to some basic concepts in industrial psychology and then proceeds to consider the job and its basic requirements. He

considers next the usual areas of study in industrial psychology, i.e., training, motivation, morale, accidents, etc. A chapter on the age of workers is also included. Orthodox presentations of these topics are made. Little attention is given to newer applications of social psychology to industrial personnel problems.

Chapters on methods of work, wages and job evaluation, nutrition and rest, and lighting and ventilation are also included. It appears that these areas should be left to the industrial engineer and more time spent on the basic psychological problems of industry. A text is needed which will start out by stating the basic objectives and functions of the industrial psychologist. Such an approach would help the beginning student and the professor who has to teach the course. Moreover, emphasis needs to be placed on methods and procedures through which industry might benefit in worker efficiency.

Gray has written a textbook which provides the basic fundamentals and concepts of the field of industrial psychology. In this respect the book is a good compendium of facts. The text, however, falls short in the presentation of the real problems of the industrial psychologist.

D. J. MOFFIE
North Carolina State College

DANIEL, ROBERT S., & LOUTTIT, C. M. *Professional problems in psychology.* New York: Prentice-Hall, 1953. Pp. xi+416. $5.50.

"Professional" is used broadly to include the problems of the teacher, research worker, and scholar in psychology as well as the problems of the psychologist who earns his living in clinical, industrial, or some other form of psychological practice. For the members of that broad audience Daniel and Louttit have collected a surprisingly varied amount of information, much of it of course from other sources, but in good part original.

Do you want to know the names and publication dates of bibliographical sources to psychological or general literature, the address of a publisher, where you can get a fractional horsepower motor, an electronic regulator, or a test? Are you interested in a definitive list of the 331 psychological journals that have been published at one time or another, in an authoritative guide for abbreviating journal titles, or in information on the preparation of various kinds of manuscripts?

Do you want to know who attended the first APA meeting, how many members the Association had in 1943, where and when the first International Congress of Psychology was held, or the names of the four psychological journals that were published before 1800? Or do you want a list of state psychological associations, information about ABEPP examinations, or a discussion of public relations in psychology?

This sampling gives an idea of the wide scope of information contained in the four sections and four appendices which make up the book. The first section is introductory. Then follow series of chapters on psychological literature, reporting psychological research, and problems of the professional psychologist. The appendices are an annotated list of reference books of value in psychology; a bibliography of journals in psychology; a list of sources for books, tests, apparatus, equipment, and supplies; and a glossary of abbreviations useful to the psychologist.

The inclusion of sections on psychological literature and the reporting of psychological research may come as a surprise to some readers who interpret the title as dealing only with licensing, ethics, interprofes-

sional relations, and other such "professional" topics. But the inclusion is a valuable one, and the authors are well qualified in these areas. Louttit, in particular, has long been one of psychology's experts on psychological literature. Both of these sections are necessarily condensed, for they cover a lot of ground. But both are authoritative. The final section, on problems of the professional psychologist, is also brief; it too covers a lot of ground, but the nature of the material does not permit an authoritative treatment. Psychologists have not yet reached full agreement on many of the problems discussed in this section, and the very nature of the problems makes it impossible for the authors to be as definitive as they could be in the earlier sections. The major problems are introduced; some of the principal points of view are outlined; and references to fuller discussions are plentiful.

For the graduate student the book consists of a thoughtful compendium of advice about collecting a working bibliography of source materials, about the publication policies and the kinds of articles accepted by each of a number of journals, about the preparation of a manuscript and how to see it through the press, about ethics, interprofessional relations, psychological organizations, the teaching of psychology, and other problems that he will encounter as a professional psychologist. For the mature psychologist the book's chief usefulness will be as a reference volume. He already knows some of the things included, so can browse through it to get acquainted with its coverage. Then it belongs on the handiest shelf, for it will be a frequently consulted reference source.

DAEL WOLFLE
Commission on Human Resources and Advanced Training

CABOT, HUGH, & KAHL, JOSEPH A. *Human relations.* Cambridge, Mass.: Harvard Univer. Press, 1953. Vol. I. *Concepts.* Pp. xxxi+333. $4.75. Vol. II. *Cases.* Pp. viii +273. $4.25.

These two volumes by Cabot and Kahl constitute the fruits of their efforts to study and teach human relations at Harvard University during the last decade. Simply stated, their approach involves two fundamental views: (*a*) that effective face-to-face human relationships can only emerge from a knowledge derived from the application of fruitful social science concepts to concrete social situations. Ostensibly, a full understanding of these relationships requires not only meaningful conceptual tools but also the skills necessary for their utilization in specific social settings. And (*b*) that the communication or learning of the concepts and the skills necessary for their application can best be achieved by the medium of group discussions of case material representing concrete social situations.

Volume I is devoted almost exclusively to the interpretation and elaboration of selected social concepts, whereas Volume II consists of 33 cases of social interaction in face-to-face situations. The presentation in Volume I takes the form of a series of discussions by the authors in which the theoretical views of various prominent social theorists and practitioners are summarized and, to some extent, integrated. At the end of each discussion, i.e., chapter, many of the writings from which these views were taken are presented as a series of readings. In some cases complete articles are reproduced and in others pertinent selections from books are given. Approximately two-thirds of the first volume consists of these readings. Because the thinking of so many

theorists is involved, and inasmuch as many of these views have been extensively reported on and evaluated in the literature, critical evaluation of specific theoretical approaches will not be attempted. As a source book of human relation concepts intended for the student and nonacademic reader, the presentation of these concepts in terms of clarity and level of discussion becomes the critical question.

It is clearly evident that an attempt to understand human interaction in specific face-to-face situations implies an idiographic or "clinical" approach. The first two chapters of Volume I are devoted to a discussion of the nature and difficulties involved in such an approach. Here the authors stress the importance of theoretical generalizations in understanding the unique event as well as a consideration of all the factors in the situation and of the relationships among these factors. They caution against distortions in social observations introduced by "habits of thought and language." In effect, the discussion summarizes the views of A. J. Lowell, L. J. Henderson, C. H. Cooley, S. I. Hayakawa, and others.

Chapters III to VII deal with concepts relevant to understanding the behavior of the individual in relation to others. Most of the theoretical conceptions considered have been cited extensively in psychological and sociological literature and thus need only be cited in passing. Chapter III, entitled "Cultural Values and Social Roles," draws heavily upon the views of Durkheim ("Anomie"), Florence Kluckhohn ("Dominant and Variant Cultural Value Orientations"), Linton ("Status and Role"), Benedict ("Continuities and Discontinuities"), Parsons ("Age and Sex in Social Structure"), and Warner and Lunt ("Social Class and Social Structure"). A discussion of the conflict between the need to belong and the individual's desire for independence in Chapter IV briefly summarizes the thinking of Erich Fromm. A number of the writings of Piaget constitute the framework for a discussion of "Our Developing Social Interactions" in Chapter V. Chapter VI considers the nature of conscious and unconscious thought processes (Freud, Mumford, etc.); and finally, Chapter VII is devoted to a discussion of the effects of anxiety and fear on social interaction (Mayo, Horney, etc.).

The remaining six chapters are focused more or less on the problems of group structure and function. Chapters VIII and IX deal with "Group Membership" and "Group Processes" respectively (Cantril, W. F. Whyte, Davis, Homans, etc.). Chapter X deals with the problem of differences in group values reflected in the conflicts between racial and religious groups in the United States. The writings of Rose, Myrdal, Lewin, Frenkel-Brunswik, and others form the basis of this discussion. The problem of modifying values and feelings is discussed in Chapter XI. Here the importance of a nondirective approach in a face-to-face situation is stressed as the primary condition for bringing about change. As one might expect, the writings of Carl Rogers form the basis of much of this treatment. The final two chapters, XII and XIII, deal with the problems of "Executive Leadership" and "Social Control and Equilibrium."

It is readily patent that the concepts discussed in Volume I—as the authors themselves point out in the Introduction—are "interrelated concepts from several . . . disciplines." In various places the interrelatedness of some of the concepts is stressed, but, as the authors indicate, no attempt is made to put forward a

"tightly reasoned system of social theory." The order in which the concepts are presented rests on the assumption that effective action in a social situation must be preceded by skillful diagnosis. Thus, the discussions in the earlier chapters orient the individual toward the problem of social observation, while the later ones are more "action" oriented. Cabot and Kahl are by no means dogmatic about the order of concept presentation; they suggest that the order can and should be varied to meet the needs of particular groups.

In the main, the selection of concepts and the order in which they are presented reflect very careful thought. In reading through Volume I, one cannot but be impressed by the authors' cogency in organizing social data. Yet, one rather serious criticism of the volume should be made. We have no quarrel with the particular social concepts selected, but rather with the conspicuous absence of any systematic discussion of the biological contributions to personality development. True, the purpose of the writers was to provide the reader with a social theoretical framework for understanding social interactions in concrete situations. But they have ignored their responsibility of not leaving the student and nonacademic reader with the distorted view that human reactions are purely a function of sociocultural factors. Some preliminary discussion of the contributions of essential structure and equipment in producing differences in intelligence, drive intensity, and the like would have gone a long way toward reducing the possibilities of such distortion. While it is critical for the reader to understand the importance of learning in the development of social interaction, it is no less important for him to understand the limitations imposed on such learning in an individual with an IQ of 70.

Perhaps the most disturbing aspect of Volume I is the level of presentation. Both volumes are clearly intended for undergraduates in integrated social science courses and nonacademic groups, of various kinds. Somewhere along the line the authors lost sight of some, if not all, of their intended readers—insofar as Volume I is concerned. In general, their discussions tend to be abstruse and, in the reviewer's opinion, will tax the comprehension of the average undergraduate and certainly individuals in many nonacademic groups. In part this is because, although the writings of many of the theorists demanded clarification and a simpler presentation, Cabot and Kahl simply summarized them in much the same language. While various Steig cartoons presented throughout Volume I serve to suggest a concern with the communication of ideas, the actual discussions stand in sharp contrast to these ingenious drawings. A greater concern for the general reader by avoiding esoteric discussions would have gone a long way toward improving the value of the volume.

On the other hand, the selection and presentation of the case material in Volume II are outstanding. The cases depict actual social interactions encompassing a wide variety of situations. Inasmuch as they are to serve as the "raw data" that are to be organized in terms of the concepts presented in Volume I, they are straightforward factual accounts. They are presented in detail, with both background material and verbatim conversations and, more significantly, without any attempt to illustrate particular theoretical points of view. Because their approach to increasing the individual's understanding of human relations rests on his learning through group participa-

tion, Cabot and Kahl selected those cases which they consistently found produced good group discussions. In effect, the cases have been "pretested."

While the two volumes were designed as a unit, it is clearly evident that their use is not restricted in this respect. The cases can be employed with other concepts (or readings) than those presented in Volume I; and, of course, the concepts in Volume I can be used in any order or with a different set of cases. As the writers indicate, "the chapters and cases are so written that they are to a considerable extent complete in themselves, and such modifications can be readily made."

The number of colleges and universities in which integrated social science courses are being introduced into the undergraduate curriculum is on the increase. There is a desperate need for especially prepared written material for such a course for first-year college students. Both objectively reported case materials and succinct theoretical discussions for the undergraduate, lacking a background of specialized courses, are required. With respect to the former, Cabot and Kahl have made a definite contribution. Unfortunately, the same cannot be said with reference to the latter. While some portions of their conceptual discussions in Volume I probably can be used for integrated social science courses, on the whole much of it falls far short of the mark.

HAROLD PROSHANSKY
Brooklyn College

WHITING, JOHN W. M., & CHILD, IRVIN L. *Child training and personality: a cross-cultural study.* New Haven: Yale Univer. Press, 1953. Pp. vi+353. $5.00.

This is the report, within a specifically defined theoretical framework, of a careful study in which 75 primitive societies are used as the individual variables. The sources of information about these societies are published anthropological reports, and are given in a classified list of references. The aim of the study is to test certain hypotheses about the effect of child-rearing practices on producing characteristic personality structures of the members of a culture. The authors have set up a series of hypotheses, and then tested them by applying to them available descriptions of the cultures concerned. These hypotheses are derived from a general behavior theory in which certain aspects of Freudian psychoanalytic theory are adapted to Hullian behavior theory.

Five systems of behavior are selected for study because they are considered basic to personality development, are subject to training and control in all cultures, and are treated in a wide variety of ways in the different cultures. These systems of behavior are: oral, anal, sexual, dependence, and aggression. For each culture the data about each of the systems were categorized according to (*a*) initial indulgence, (*b*) age of socialization, and (*c*) severity of socialization. Within these categories the classes of behaviors were defined in such a way as to maximize the objectivity of classifications, and to facilitate making useful distinctions. Three judges, working independently, rated and ranked the data.

The Freudian concept of fixation is considered basic, but it has been modified by division into positive and negative fixation. Evidence for both kinds of fixation is studied in each of the five systems. Positive fixation is expected to result from satisfactions experienced when drives in any behavior system are indulged.

Negative fixation should occur in a behavior system when training interferes with natural behavior tendencies. Several other concepts, such as "habit potential," "custom potential," and "custom complex," were defined as important in dealing with the cultural variables.

The personality characteristics studied are based on customs for which there was generally good information and for which, according to the authors' theory, there was expectation of a relation to the child-training practices studied. These characteristics are the attitudes in a society toward illness, its causes and cures, and toward death. Customs relating to illness were divided into: explanations of illness, performance therapies, avoidance therapies, and fear of others. Each of these was in turn subdivided according to the five basic systems of behavior. The data on illness from each culture were then classified by two judges who were kept in ignorance about the child-training classifications.

Interrelationships were studied among the various aspects of child-rearing practices, and between these and the personality characteristics of the adult members of the cultures, as evidenced in their customs in relation to illness. In many cases the theoretical hypotheses were confirmed or supported; in others the expected relationships were not found. The failure of verification of some of the hypotheses could be due to inadequacy of the data, which are often lacking in information pertinent to this inquiry. The findings are interesting and present a very challenging array of problems for future testing, by additional cultural studies and by experiment.

It is difficult to summarize the findings from the study, as they are so bound up with the context of interrelating variables and theories. But some examples may be given. The authors feel justified in their separation of positive and negative fixation. Negative fixation was most clearly demonstrated: it was shown in "the effects of anxiety developed during the period of socialization for each system of behavior." Positive fixation seemed to be related to a later period of childhood and to anal and sexual satisfactions. "Viewed as a whole, the evidence for a lasting effect of child training practices on personality is more striking and consistent for the three . . . systems—oral, dependence, and aggression." There was some evidence that guilt arises in the process of socialization as a consequence of identification with the punitive parent. In a very ingenious treatment of the data relating to the origins of unrealistic fear of other persons (paranoia) the authors "found that the fear of others is primarily associated with anxiety about aggression." Although the correlational method does not permit determination of the direction of the cause-effect relation, the authors feel they are justified in concluding that child-training practices do at least in part influence adult personality.

An incidental finding of interest is a comparison of child-training practices in midwestern urban middle-class families and the primitive cultures. By comparison, the Americans were exceptionally early and severe in toilet training (anal), and quite severe in weaning (oral) and in general socialization (average of five systems). Only in the socialization of aggression are they close to the norm for primitive cultures. However, the evaluation of these tendencies should be made in relation to their place in the total cultural pattern, and the authors explicitly make no recommendations regarding them.

This book has much to contribute, methodologically and theoretically, to the systematic treatment of the development of personality. But it is very difficult to read. The findings are buried and easily lost in the discussion and the tedious (though often important) qualifications of theoretical presentations. Perhaps this difficulty is unavoidable in working out this kind of complex general behavior theory. But it seems to this reviewer that a simpler style could make the theory a little more readily available and generally understood.

NANCY BAYLEY
University of California, Berkeley

ROSEN, JOHN N. *Direct analysis: selected papers.* New York: Grune & Stratton, 1953. Pp. vii+184. $3.75.

The regard, respect, and human warmth for the mentally sick individual that John Rosen conveys in his public appearances are well reflected in the nine papers which have been collated into this slim volume. Eight of these papers have seen prior publication or presentation before psychiatric audiences, but the first paper, "Direct Analysis: General Principles," was specifically prepared for this book.

There are two basic premises from which Rosen proceeds: first, that unless physical pathology can be clearly demonstrated, psychoses are psychogenic in origin and are treatable by psychologic means; and second, that the nuclear core of the psychotic response is formed in the oral stage of infant dependence upon the mother. The effectiveness of direct analysis lies in the capacity of the therapist to enter without fear directly into the patient's psychosis and to clarify for him the dynamics of this oral deprivation by direct psychoanalytic interpretations as well as by providing some of the absent love. Once reality contact has been firmly established, the therapy proceeds along more traditional psychoanalytic lines. On the basis of his experience, the author categorically asserts that he knows of no psychogenic psychosis that cannot be significantly improved by psychological therapy (i.e., psychoanalysis for Rosen). The author seems to have need for a rather uncritical, uncluttered, orthodox Freudian psychoanalytic theorizing to lend "scientific respectability" to his superb skill in interpersonal relationship therapy and to enable him to communicate to others what he believes to occur in the therapy situation. As an experimental method for studying the psychotic process, Rosen's techniques may prove extremely valuable. The limitations of application of this procedure to the massive problem of the psychotically ill are self-evident.

The interview and case material provide a fair amount of source material on the practice of direct analysis. The book is well worth reading not only for its contributions to the psychopathology of psychosis but also for its quality as a personal document.

M. ERIK WRIGHT
University of Kansas

VITELES, MORRIS S. *Motivation and morale in industry.* New York: Norton, 1953. Pp. xvi+510. $9.50.

Viteles has undertaken, in this volume, to synthesize the data and reconcile differences from the ever increasing investigations of the "will-to-work" in industry. He has done an acceptable job in terms of the limitations of the studies available and the diverse orientation toward the problems of motivation in industry that the investigators on whose work he relies have. In doing this he

has produced a much needed review of the literature in this area for research workers and a handbook for personnel-minded members of management.

The book is directed primarily to members of management who are responsible for the development and administration of personnel policies. The emphasis throughout is on applications of research findings to problems of motivating employees. Viteles has developed a reasonably consistent set of principles for this purpose and has presented specific suggestions for their application. He also has pointed out inadequacies in available data and emphasized the need for further research.

In addition to affirming the importance of security, participation, status, and role in employee motivation, the author has re-examined the problem of economic motivation and has rescued it from the limbo to which it has been consigned by the Hawthorne investigation and certain attitude surveys. He has phrased properly the problem of financial incentives versus human relations in the motivation of workers: employees want both, and the problem confronting management is an integration of the two.

Viteles relies for his data on investigations classified roughly as "experimental" studies and attitude surveys. Typical of the former is the Hawthorne investigation. Data from attitude surveys represent a wide range of approaches, from those of Houser in the late twenties to the General Motors MJ Contest and work of the Michigan Survey Research Center and the National Industrial Conference Board. Most of the studies have appeared within the last five years. The investigations, for the most part, were conducted in industrial situations.

In spite of the excellent synthesis of these investigations, this reviewer is concerned with Viteles' failure to evaluate critically certain investigations on which he relies for his conclusions. It is true that he has an excellent chapter on the weakness of attitude survey methodology. Yet results of studies by the Opinion Research Corporation, Rose's study of workers' attitudes toward labor unions, and others are presented without making specific their limitations. Viteles cannot assume that the audience for which this volume is primarily intended, managerial personnel, will be well enough acquainted with these investigations or sufficiently sophisticated to evaluate their results without a specific statement from the author as to their limitations. In contrast, Viteles has been quite explicit as to the research weakness of the investigations dealing with group decision making in motivating employee behavior.

As indicated earlier, this volume is intended primarily for administrators concerned with personnel policy. This reviewer is somewhat apprehensive that the administrators who read this book will receive the idea that developing the will-to-work is primarily a matter of techniques rather than dependent on a fundamental philosophy of the dynamic relationship between a man and his job. There is a wide discrepancy between regarding "security" as a means of increasing worker productivity and regarding "security" as a way of living essential to the well-being of both the individual worker and his employer. The latter concept may be implicit in Viteles' treatment of incentives but it needs to be made explicit.

The author usually keeps in view the audience for which he is writing. The style is reasonably straightfor-

ward although the wealth of material cited at times results in the impression that the volume is an annotated bibliography. Statistical terminology at times will confuse the lay reader especially since no explanations of the concepts are offered.

Motivation and Morale in Industry has real significance for industrial psychology. It is the only comprehensive and compact source of bibliographical data in this area available to psychologists working on industrial problems. Even more important, it emphasizes the centrality of the problem of motivation in industrial psychology. It is significant that Viteles has chosen to write on this subject rather than to revise his *Industrial Psychology* published in 1932. It is an indication that psychologists concerned with industry are now moving away from a primary concern with the peripheral problems of employee selection, training, vision, and job analysis to the central problem of the work situation—the dynamics of behavior within the industrial organization.

WILLIAM MCGEHEE
Fieldcrest Mills, Inc.

WALTER, W. GREY. *The living brain.* New York: Norton, 1953. Pp. 311. $3.95.

Starting with a delightfully written, simple story of the evolution of the nervous system and the emergence of the brain of man, Walter ends with some thoughts on the brain of tomorrow. He pays high tribute to Pavlov and Berger, whose contributions to conditioned reflexology and electroencephalography, respectively, are landmarks in our understanding of the brain. He attempts a convergence of the notions resident in these seemingly diverse areas. A brilliant thinker and writer, Walter does not hesitate to highlight his own role in the tremendous progress in electrophysiology in the last decade or so. For one looking on *de novo*, the impression might be gained that electroencephalography has been brought to its present state of development by the author singlehandedly.

The EEG and its elaborations by automatic analyzer and toposcope are looked upon as a suitable mirror for the brain's physiological activities, especially for detecting the significance of patterns which may correlate with the psychological phenomena of perception, learning, and personality. Certainly some interesting ideas are presented. The reviewer shares with Grey Walter the hopefulness of *rapprochement* in the realms of the physiological and psychological, but at best it will be a slow and laborious process. This is a book of ideas and hunches, flavored perhaps more with wish fulfillment than with empirical fact, but nevertheless an exciting account which psychologists might well read with profit.

Walter is greatly impressed with the subjective effects of flicker stimulation, and like others sees in photic driving a hopeful device for manipulating brain rhythms in frequency, phase, and space distribution with the goal of testing the hypothesis that brain rhythms serve a scanning function similar to that in television. Two chapters, six and seven, entitled "Learning about Learning" and "Steps from Chance to Meaning," ought to prove of interest and value to the psychologist, for here elements of neurophysiology, communication theory, feedback, and scanning mechanisms are brought to bear on the perceptive-learning-memory process. Much of Walter's thinking on such matters will seem somewhat more cavalier than comprehensive, and stems from analogical conceptions developed through

the construction of ingenious electromechanical models. The most elaborate of these is Machina Speculatrix, a bug-like toy, which has built into it potentialities for responsive behavior described as exploratory curiosity, sensory discrimination, negative and positive tropism, and other properties called discernment and self-recognition. With such a device he has identified what he believes are seven steps from chance to meaning. The circuit diagrams for a nerve model, M. speculatrix, and a conditioned reflex analogue are presented in appendices. All in all it is an interesting and provocative book.

DONALD B. LINDSLEY
University of California at Los Angeles

FESTINGER, LEON, & KATZ, DANIEL. (Eds.) *Research methods in the behavioral sciences.* New York: Dryden Press, 1953. Pp. xi+660. $5.90.

Because of the large number and variety of variables that may affect the phenomena they study, psychologists have been forced to pay particular attention to problems of research design and techniques of observation and measurement. *Research Methods in the Behavioral Sciences* is devoted to the consideration of these problems as they are faced by the investigator who is concerned with human behavior in its social setting.

The general attitude of the authors influences the organization and content of the text in at least two different ways. In the first place the methods discussed are conceived to apply widely to studies of social behavior regardless of the discipline—anthropology, psychology, sociology—to which an investigator formally claims allegiance. The emphasis is upon the type of question asked rather than upon the arbitrary division of the social sciences. It is for this reason that the text has in its title the term "behavioral sciences." In the second place, the authors feel strongly that progress in the investigation of man's social behavior is most likely to be achieved by use of the basic logic of scientific methodology. This is the logic upon which research design in all sciences depends, hence, ". . . there are probably no social-psychological methods as such."

I would give an incorrect impression to imply that this text is primarily a discourse on the logic of scientific method illustrated with examples from research in the behavioral sciences. A distinction made by the authors between methods of research design and techniques by which a design may be put into effect is analogous to the military distinction between "strategy" and "tactics." Accepting that scientific logic is common to all sciences, the editors emphasize that " . . . specific techniques and approaches will vary, depending upon the subject matter . . ." under investigation. It is in their tactics that the behavioral sciences differ from other sciences.

The most important contribution of this text lies in the systematic manner in which the techniques available to the behavioral sciences are presented. The text follows the sequence of operations confronting the research worker from the time the question he is asking has been clearly stated to the time when he is ready to present a report on his efforts to answer it. A major portion of the book is devoted to procedures for sampling a population to be studied, and to techniques of collecting and analyzing data. In many cases techniques are described in the detail which is usually associated with a handbook. In this respect, the text is most useful to those who wish to check their own procedures as well as

those who are in the process of developing new research skills. The latter should understand that just because the text is so systematic in its organization does not mean that all methodological problems in social psychology have been solved.

It is understandable that, in attempting to be systematic, the authors have not always succeeded in introducing originality into their subject matter. However, because they have attempted to be systematic, they have included discussions of matters of importance to scientific method which have received little attention in similar texts in the past. With a bit more daring—or, perhaps, more space—they might have ventured to present more fully the logical bases of different patterns of research design. They perform a valuable service in discussing the basic assumptions and logical requirements that underlie the research techniques they are discussing. Problems of objective observation are considered in terms of the processes of hypothesis construction and of research design which precede them in the sequence of steps through which the investigator must pass. Systematic observation involves some form of measurement no matter how relatively crude it may be, and it is encouraging to find that a discussion of the theory of social measurement leads to an emphasis on the valuable roles which nonparametric methods of analysis may play in analyzing data of the type which most measuring instruments in the social sciences now provide.

The systematic coverage of the field which characterizes this text persists through the final chapter, where it is pointed out that the role of the social scientist frequently does not end with the completion of a research project. Questions of applying research findings arise, bringing with them problems of a technological nature which are themselves in need of research. Since little knowledge regarding such problems is now available, the best that can be done is to discuss experiences in this branch of psychotechnology and to make a case for including such considerations in future research and training programs.

Research Methods in the Behavioral Sciences stands up well under the criticisms that are frequently leveled against texts to which a number of authors have contributed. The excellent integration of its thirteen chapters suggests that very careful and detailed planning must have preceded the actual writing. There is much less duplication of material than is usually found in such collaborative endeavors. Although the results of a Flesch count—old style or new—are not available, the general level of readability appears to be good and within the grasp of advanced undergraduate or graduate students.

For their present purposes the authors have drawn upon ". . . a limited pool of methodological research findings, to which they have themselves contributed." There is never a doubt that the authors have other than the same general orientation toward the field they are discussing. With few exceptions they are affiliated at present, or have been in the past, with the development of social psychology at the University of Michigan. This has advantages as far as the presentation of a coherent, well-integrated point of view is concerned, but in all probability it is a characteristic of the text which will bring comments from readers who would like to have seen material included that does not appear.

A lesson for some to learn from a

thoughtful appraisal of this text is the unsatisfactory nature of attempts to define a science in terms of the subject matter it investigates. Psychology can claim to be a science in so far as psychologists employ scientific methodology, based, as it is, upon certain well-established principles of reasoning conforming to certain strict requirements and accepting certain articles of faith. The authors have shown how the experimental and nonexperimental methods of science can be called to the aid of those who are interested in man's social behavior. This achievement will not come as a surprise to those who have followed the development of social psychology over the past number of years and know how forcefully and consistently Dr. Katz has pressed for careful attention to methodological problems. The present text illustrates that he and his associates have gone a long way in this direction, yet recognize that there is still a good distance to cover.

ROGER W. RUSSELL
University College, London

SARASON, SEYMOUR B. *Psychological problems in mental deficiency.* (2nd Ed.) New York: Harper, 1953. Pp. x+402. $5.00.

Dr. Sarason has written a very good, scholarly book. Professional students in the social and biological sciences and relatively well-informed laymen should find it unusually readable. Some readers may react against arguments by assertion and the sometimes polemic and hypercritical style, but these characteristics do not detract from the solid merits of the book. One gets the impression that it was written by an informed, critical psychologist who was stimulated by his experience to prod us out of the lethargy and pessimism which are so frequently found among professional workers in the field of mental deficiency.

The book does not attempt a thorough coverage of the etiologies, characteristics, and treatment of the great variety of pathologies that are thrown together in this so-called field. Rather, it is most valuable for its points of view, the discussion of research, and the questions raised. I see Sarason's book as one of the opening guns in a revolution which may overthrow or, hopefully, dismember this field of mental deficiency. For, if I read Sarason right or without too much projection, he is telling us that the mental defective is not what he seems or what the IQ says he is, that not all mentally defective children are *mentally* defective, and that many things can and should be done about their problems. In other words, the measured intellectual level is *not* the problem but is only a symptom. Sarason's emphases upon cultural problems and the effects of environmental stimulation and psychotherapy are important and sound.

The net result of Sarason's arguments should be the recognition that the field consists of many special pathologies, organic, genetic, and psychogenic, which should be diagnosed in terms of etiology and prognosis under appropriate treatment rather than in terms of intelligence test scores. He fails to make a complete break with the past, however, and the reader may get the impression that the term mental deficiency still somehow describes a meaningful category or homogeneous group.

The frequent use and implications of the term "garden variety defective" bother me. The term is used to refer both to cases of inherited deficiency and to psychogenic or cultur-

ally determined cases. The term is badly chosen as it is not at all consistent with Sarason's argument for individualized understanding and treatment, and it is rather meaningless and offensive. Perhaps it is a quibble to question such statements about projective techniques as "in recent years the psychological pendulum seems to have swung from the Binet to the Rorschach as the basis for a diagnosis of mental deficiency" (p. 242). No evidence is given and Sarason's discussion would seem to contradict it.

The final chapters are concerned with the important problems of dealing with parents, the question of institutionalization, and the inadequacies of professional training. One can only applaud Sarason's position and treatment of these problems. On the whole, the book will undoubtedly improve the stature of professional work in this broad but neglected field.

KARL F. HEISER
The Vineland Training School

COMMITTEE ON COLORIMETRY OF THE OPTICAL SOCIETY OF AMERICA. *The science of color.* New York: Thomas Y. Crowell, 1953. Pp. xiii+385. $7.00.

The principal contribution of this book may lie not so much in the factual data presented as in the exposition of "color" as a psychophysical concept. This concept is of such importance and broad application that it will undoubtedly have considerable influence on scientific procedures in other research fields of the special senses. Even if it took ten years to hammer out this basic idea, the results may compensate for an otherwise disappointing publication.

America's foremost specialists in color collaborated in producing this most authoritative book on color—authoritative but inhomogeneous. Perhaps the work should have been presented as "The Present Status of Color as Viewed by Twenty-three Specialists" rather than as a definitive work. It is still an interim report, a combination of textbook, handbook, essay, and committee paper. It covers the principal phases of color: physical, psychophysical, and psychological; it gives a miniature history of optics, a one-reel travelogue through ancient civilizations, and the usual anatomical digest. The rapid run-through of instruments, methods, and standards is given with an economy suggesting that the bell is about to ring and the students are rustling their notebooks. As a book, it is marked by the lack of an over-all plan, unevenness, discontinuity, minor errors, and many irritating kinks in arrangement.

Nevertheless, it will stand on the right-hand bookshelf for many years because it has features which will not soon be supplanted. The references are comprehensive and well selected, especially in the psychological field. Professional workers will appreciate the most complete assortment of colorimetric tables that has yet appeared in one volume. Its major offering, "The Concept of Color," is thoroughly expounded for the reader who is willing to make the effort, though the material is scattered in the Introduction and parts of Chapters 2 and 7. The diagram in Figure 5 is invaluable to this explanation and might well have been used as the frontispiece of the book. The glossary-index will be most useful to visitors from other scientific fields. Excellent color plates help to clarify the psychological organization of color sensations into three dimen-

sions. The large, diffuse field of color phenomena has been painstakingly screened and organized in the two chapters on "Psychological Concepts," though it is very difficult for the reader to find his way through such diverse material without a special table of contents.

If the book has failed to reach its goal, we know it is not for lack of prodigious effort on the part of a number of the editors, for lack of erudition, or for lack of physical resources. Perhaps the difficulty lies in the times. Our best scientists are pushed at top speed on their jobs, and there is less time than formerly for the repose in which to reflect on the broader aspects of a part-time, unpaid assignment which must be taken out of family and social life. Also, all scientific fields have expanded so rapidly that it has become increasingly difficult for specialists to encompass them editorially. It is interesting to consider what form the book might have if the editor of the *Scientific Monthly*, for example, were asked to plan and supervise the next edition.

It is curious that a group of scientists with international scope should write with such an insular viewpoint. This is strictly "the American science of color." There is no suggestion here of the enlightening theoretical approach to color space developed by Schrodinger and Bouma, or of current developments on the Continent. Nor would the reader suspect the major contributions which have been made in psychophysical research methods by the British in their several institutions.

The neglect of the British field of research emphasizes an omission of fundamental importance in this as well as in most other recent books on color. Much space is devoted to methods, formulae, tables, and statistics for colorimetric work; but scarcely a paragraph can be found on the corresponding methodology and tools of experimental psychology. The superb instrumentation which has been developed by the physical and electronic technician has made it possible for anyone with a few thousand dollars to lay claim to be a "colorist." This can easily lead to the dangerously naive viewpoint that psychological methodology is unnecessary or perhaps may be expected to "just come naturally."

A book which is designed to fill such an important need deserves a future edition in which the psychophysical and psychological sections are strengthened to balance the physical, in which the pertinency of other sections is reviewed, the inclusion of now-omitted sciences proposed—and with the whole plan supervised by a professional editor-in-charge.

DEAN FARNSWORTH
Visual Engineering Section,
U. S. Naval Submarine Base,
New London

MORENO, J. L. *Who shall survive?* New York: Beacon House, 1953. Pp. cxiv+763. $10.00.

This is a very large book. It has over 100 pages of preface before it gets to page 1. Afterwards it still runs to over 700 pages. In the long preface the author tells us that:

1. Sociometry is a religion as well as a science (e.g., page xv): "I tried to do through sociometry what religion without science has failed to accomplish in the past. . . . "

2. The author is a genius (e.g., page xxxvii): "All my books (nine) published between 1919 and 1925, were anonymous. The natural state of genius is anonymity . . . *The*

Words of the Father I wrote with red ink on the walls of an Austrian castle."

3. Sociometry is the final development (e.g., page lxvii): "Actually, I have written two bibles, an old testament and a new testament, *The Words of the Father* and *Who Shall Survive?*"

4. Psychoanalysis is finished (e.g., page liv): " . . . the psychoanalytic system was stillborn to start with. . . . The psychoanalytic couch has become a piece of furniture in the sociodynamic field of the psychodramatic stage."

5. Group therapy, group dynamics, and, it seems, all work on small groups and large groups are developments of, or secessions from, the sociometric movement (pages lxix, lxx, and xcvii to cviii).

The body of the text is devoted to an exposition of the theory, methods, and accomplishments of sociometry. While it is difficult to give a brief review of over 700 pages, I will try to list the major points which are made.

1. The most important concepts of the sociometric system are spontaneity, creativity, Tele, the social atom, and underlying social networks. There is considerable discussion which conveys to the reader a vague feeling concerning the meaning of these terms. There are no very specific or rigorous definitions.

2. The basic techniques of sociometry are psychodrama, sociodrama, role-playing, group psychotherapy, and the sociometric test. Only the discussion of the last of these is at all elaborated in detail. There is little more than brief mention of the others. The only empirical data presented concern the sociometric test, the essence of which is asking people with whom they want to associate.

3. A major portion is devoted to detailed description of two studies using this sociometric test. Data are presented to show that the distribution of choices is different from chance, the pattern of choices changes with age, attractions among persons are not always in accordance with formal or existing associations, and that if groups are made up in accordance with the desires of the members, these groups hold the members' interest better.

To this reviewer it seems that there are many pages devoted to stating how important sociometry is. There is, however, little space devoted to any convincing empirical demonstration that these claims are valid.

LEON FESTINGER.
University of Minnesota.

MORSE, NANCY C. *Satisfaction in the white-collar job.* Ann Arbor: Univer. of Michigan, 1953. Pp. viii+235. $3.50.

It has long been part of the folklore of industry that a satisfied employee is a productive employee. Recent investigations of the Michigan Survey Research Center have questioned this assumption. The indication that there is no one-to-one relationship between satisfaction and productivity has been disturbing both theoretically and practically. Morse has attempted in this volume to reduce theoretical tension by theory construction.

Satisfaction, according to Morse, is "the individual's perception of the tension-level within him" (p. 32). This tension-level in turn is a function of what an individual wants and what he receives (environmental return). Morse further considers the amount of satisfaction derived from a given activity as a separate variable from motivation, i.e., "the willing-

ness to expend energy in a given activity" (p. 11).

Productivity is, however, a much more complex phenomenon to predict than satisfaction, as it is dependent on more factors. Morse postulates at least seven. The only factor which productivity has in common with satisfaction is the strength of needs. Accordingly, individuals will experience high satisfaction and be high in productivity if they have strong needs which are "predictably" related to productivity and where environmental return from the job is high. If environmental return is low, productivity may be high but the amount of satisfaction will be low.

Morse emphasizes throughout the volume that the hypotheses concerning the nature of satisfaction and of productivity and their relationship are tentative and will require further investigation to verify, modify, or refute. As an indication of their possible validity she offers evidence from the analysis of interview data with 742 clerical employees and 73 first- and second-line supervisors in a large corporation. She points out the limitations, and possibly atypical nature, of this population.

Her evidence, as far as it goes, supports her hypotheses concerning both satisfaction and productivity. The limitations of her data, in addition to population limitations, seem to be that the investigation was not originally planned to test hypotheses concerning these two variables. For example, her indices of job satisfaction are limited to only three measures. It seems that data collected for some other purpose were analyzed subsequently to throw light on the hypotheses presented.

She offers no evidence for the statement that the amount of satisfaction is a separate variable from motivation. In fact, her definition of motivation as well as of morale differs from the ones usually found in texts on the subject. An author has the privilege of defining the terms. But unless this serves some useful purpose, the use of familiar terms in a different context tends to obscure rather than clarify meanings.

Her evidence suffers from the weaknesses inherent in the interview method of securing data on problems of motivation, morale, and satisfaction. This is acknowledged tacitly in the discussion of the differences found between employees who receive general supervision as contrasted with closely supervised employees. The former were less enthusiastic about working for the company. This could have arisen, according to Morse, because "they maybe are in a situation which demands less strict conformity, they are more free to express reservations and criticisms" (p. 142). It seems that for a critical test of her hypotheses, interview data will have to be supplemented with data collected by other means.

Morse has, however, taken time out from collecting data to develop theory. It is the opinion of this reviewer that too little theory development has characterized the investigations psychologists have made in industry. The result has been a collection of unrelated and frequently contradictory data. A real need for advances in industrial psychology is good theory making. Morse has taken a step in the right direction.

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